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ON SOME POINTS OF IMPORTANCE TO ANATOMISTS[1]

IT has been said that the president of a scientific association should not burden its meeting with an address in case the communications to be made to it are numerous and of value. If this be true it seems to me that we have reached a stage at which silence on my part would be appropriate, but I can not resist the temptation to exercise my right to speak, for a few minutes only, regarding some points which I consider to be of vital importance to our profession and to this association.

I think we have safely passed the pioneer stage in the development of scientific professions in America and it is unnecessary now to formulate the forces which have brought this change about, for they are known to us all. However, during the development of a science, it is well for the workers in it to meet from time to time to pass judgment upon the recent progress that has been made. Their approval, given on such an occasion to an investigator, is a most encouraging stimulus and election by his colleagues to the presidency of such a meeting is the highest honor a scientist can receive. I accept with gratitude this elevated post to which you have appointed me and regard it not as approbation of myself, but rather as a mark of appreciation of the co-workers, whom I have had the good fortune to have associated with

[1] Remarks by the president at the twenty-first meeting of the Association of American Anatomists, New York, December 27, 1906.

me in Baltimore, and of their contributions to anatomy.

More than a century ago the status of anatomy in America compared favorably with that in Europe, but the degeneration of medical education which followed rapidly and successfully pushed anatomy into an inferior position. This decay in medical instruction reached in America as low a level as the civilized world has perhaps ever seen about the time of our civil war. At this period the chair of anatomy was almost always used as a stepping-stone to that of surgery and under these conditions the quality of the teaching was rarely good. A certain type of surgical anatomy developed from this combination and but very few contributions to the science were made. Too often, however, there was a mere exploitation of the chair of anatomy, the teaching was poor, and the practical work in the dissecting room was neglected. Since then there has been a gradual improvement in medical education, due largely to the cultivation of its underlying sciences. During all those dark years, however, there was one place in which the light of anatomy shone continuously; thither Caspar Wistar carried it and there the Wistar Institute is located.

Caspar Wistar, the second professor of anatomy at the University of Pennsylvania, did much to perpetuate the good traditions of Benjamin Franklin, John Morgan and William Shippen. He was a man of great influence, took a lively interest in natural history, and made many anatomical specimens, some of which may still be seen in the Wistar Institute. He wrote an excellent text-book of anatomy, which was used by many students. He was succeeded first by Physick and then by Horner, who made a number of important discoveries in anatomy. Horner was followed by Dr. Leidy, who held the chair for nearly forty years. During all these years the chair of anatomy became notably conspicuous on account of the luster shed upon it by the eminence of its occupants. The greatest of this brilliant group was Leidy, in fact he was the greatest teacher of anatomy to medical students this country has seen. His ideals were of the highest and his scientific discoveries were numerous and accurate, contributing much to comparative anatomy and zoology. The good influence he exerted upon the various institutions in Philadelphia has been extended over the nation through this association, of which he was one of the founders. It is also fitting, and by no means accidental, that the Wistar Institute is located in Philadelphia. Few, perhaps, are aware of what has really taken place at this great foundation for scientific anatomy. Thanks largely to the far-seeing policy of its present director, Dr. Greenman, the first division of its staff has been manned by the ablest investigators in neurology, and the Institute has been made the central institution for America by the Commission for Brain Investigation appointed by the International Association of Academies. I sincerely hope that our society will give full support to the Wistar Institute, for its work will be national as well as international.

On account of the low ebb of medical education at the period mentioned a certain dividing of the ways occurred, best illustrated, perhaps, by what happened at Harvard. At this college there fortunately appeared the ablest anatomist this country has yet produced, but the Medical School saw fit to duplicate his chair for reasons that are not clear to me. There existed in Harvard College and Harvard Medical School, side by side, Jeffrys Wyman, the scientific anatomist, with but few students, and Oliver Wendell Holmes, the poet anatomist, with many of them. This unwise arrangement, it seems to me, de-

layed the revival of sound medical education in this country for a number of years. However, the presence of Jeffrys Wyman in the Hersey chair of anatomy was of the greatest significance in founding the American school of zoology. Wyman graduated from Harvard Medical School in 1837, and after having been Warren's demonstrator of anatomy, succeeded him in 1847. During the following quarter of a century he made numerous important discoveries in comparative anatomy and embryology and contributed also to teratology and ethnology. The loss of the influence of this great philosopher and teacher upon medical students has been one of the misfortunes that medicine in this country has sustained. But it was in zoology in America that scientific anatomy was temporarily preserved and extended rather than in the departments of anatomy in the medical schools. The American anatomists should emulate Jeffrys Wyman and our first president, Joseph Leidy.

Under the conditions which prevailed it was quite natural that the better work of Europe—the work of anatomists like Blumenbach, Ernst Heinrich Weber, Meckel, Johannes Müller, Schwann and Kölliker —barely reached this country, for the little anatomy that was cultivated subserved the surgical art. This arrangement may possibly have been beneficial as a training school for surgeons, but it was so bad for anatomy that as a science and as a profession it gradually fell into disrepute among most of the people. This conception of anatomy as a mere maid-servant of surgery is still entertained by some of our colleagues in other sciences. In nearly all of the medical schools anatomy settled down to a dead level, the so-called 'practical,' and, during the second half of the past century most of the progress made in Europe found its way to America not by way of American anatomists, but through our zoologists, pathologists and physiologists. Fortunately, many of the latter have kept their membership in this society, for while this association consists of anatomists, a perusal of the list of members shows that some are also distinguished as physicians, surgeons, physiologists, pathologists, zoologists, anthropologists or psychologists. This I consider to be a fortunate circumstance, for it will prove to be a most potent factor in the reorganization and development of anatomy in this country and in its consequent broadening influence upon medical education. However, the catholicism in our society is not properly appreciated by educated people in America, for we often hear it said that our more prominent members are not anatomists, but biologists or something else. Let me illustrate: probably the most typical anatomist of us all, a man of the widest culture and a profound scholar, a scientist known as an anatomist the world over, a member of our executive committee for eight years, a founder of the *American Journal of Anatomy* and my predecessor in office, has been wrested from our ranks and called a zoologist by a recent writer in his study of American scientists. That a single writer should do this would be of no special importance, were it not that this view of the scope of anatomy is entertained by so many Americans of prominence that it interferes very much with the development of our science as a profession. Those who hold this narrow and perverted view of anatomy can not be familiar with its history, its present status in Europe, nor its recent development in America. It is the duty of the members of this association to correct this erroneous conception of anatomy by precept and by example. I have full confidence that this can be done with ease.

That anatomy played so important a rôle in the development of our school of zoology

(was absorbed in it, some will say) while it was fossilizing in medical schools can be viewed as a fortunate condition from a number of standpoints. For us it hastened the destruction of certain traditions, which can now be ignored while we are constructing a new anatomy and establishing a new *modus vivendi* with the medical disciplines. With this change we are placing ourselves in a new and a better position than ever before. While anatomy is well represented in college and university departments not connected with medical schools, we must look for the highest development to anatomy in connection with medical education. In order to be more effective in the training of scientific physicians, we are gradually making our anatomical instruction more and more inductive and this naturally reacts upon the instructor in a beneficial way.

To bring about the desired reform it is necessary to have represented in an anatomical department, even in a medical school, all which naturally belongs to this science. The study of anatomy begins with the cell, ends with the entire individual, and includes man. In fact the greatest anatomical problems almost always involve a consideration of human anatomy. The teachers and students in an anatomical department should be given a free hand; they should not be retarded by arbitrary lines; they should dissect sometimes with the scalpel, sometimes with the microtome knife; they may look through spectacles or through the microscope and they may study the arm of a human embryo or the negro brain. In other parts of the world this liberty is a self-evident necessity and has always been granted. It follows from what I have said that an anatomical department must include histology, histogenesis and embryology; in a medical school it must cover vertebrate anatomy in the fullest sense. In general, due to the influence of this society, an unrestricted anatomical department has found its way into nearly a dozen important universities during recent years. Among the universities in large centers those in this city are the only ones in which the scope of the anatomical departments is still limited, since here histology and embryology are not included. Our wandering society, meeting as it does, in different portions of the country, will be, I believe, a great force in helping to perfect and to extend anatomical departments.

This is not the time to enumerate the really good anatomical departments, nor those that have been markedly improved in recent years, but I must not fail to note the great advancement which has been made in our state universities, due to the enlightened policy of their presidents, who are of the opinion that a professor of anatomy should be a specialist ranking high in his profession. It is safe to say that those departments in which the staff is actively engaged in scientific research are contributing most to medical progress and are exerting the best influence upon medical students. Yet, American anatomical departments taken as a whole are rendering an unsatisfactory account of themselves, and it is eminently desirable that this should change. In our wanderings as an association during the last dozen years we have had good opportunities to witness the improvements and growth which have taken place from year to year in the better universities. During this period we have met at Columbia three times, and it is a pleasure to me to acknowledge to Professor Huntington the obligations of this society for the splendid example he sets before us.

It is stated in our constitution that "the purpose of this association shall be the advancement of anatomical science." I firmly believe that such advancement can be made through scientific investigation only,

but we must provide a suitable atmosphere for our investigators. It seems to me that certain conditions, which are necessary to make a good atmosphere, are, as yet, lacking in many institutions. Probably the most serious defect in our anatomical departments is due to the appointment of men in active medical practise to the chairs of anatomy. Unlike professional anatomists, they rarely have the time to devote to teaching students, nor the requisite training to enable them to develop the department properly, and anatomy necessarily suffers. However, I regard it as fortunate that circumstances have placed us in a position from which there is no retreat. To carry on the campaign, now so well started, we must have many more productive anatomists. In order to obtain them and to make the efforts of our present investigators more effective, we must use all our influence to bring the greatest opportunities and the best men together. A highly cultured community naturally desires the ablest man. My earnest hope is that those in authority in various communities will recognize that our idea of the scope of anatomy is correct, and that they will seek productive anatomists, when vacancies occur, so that our grand science may be raised to the level it has always held in Europe.

FRANKLIN P. MALL

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SOME PHASES OF PREHISTORIC ARCHEOLOGY[1]

THE American field for anthropological research is so wide and so fertile that it not only monopolizes the attention of specialists at home, but also attracts to our shores numerous foreign investigators. For attestation of this fact, one has but to cite the fourteenth International Congress of Americanists held in the city of Quebec last September. The same congress had convened in the new world twice before, once in New York City and once in the City of Mexico, the remaining sessions having been held in various European cities.

When foreign savants take such a deep interest in our own problems it is fitting that we should reciprocate by at least an occasional survey of the foreign field. In looking over the list of vice-presidential addresses read before this section, I find that two such surveys have already been made.[2] The address of Professor E. S. Morse, entitled 'Man in the Tertiaries,' was a powerful argument in favor of the existence of man's ancestors in Tertiary times. Fifteen years later Dr. Thomas Wilson chose for his subject, 'The Beginnings of Prehistoric Anthropology.'[3] He not only had something to say about Tertiary man, but also covered the paleolithic and neolithic periods. In the more than seven years that have elapsed since Dr. Wilson's address was read, much progress has been made in the prehistoric archeology of Europe. This is especially true concerning our knowledge of the *eolithic question* and of *paleolithic art* in so far as it has to do with engravings and frescoes on certain cavern walls. In fact, eoliths and paleolithic mural decorations were not even mentioned by Dr. Wilson. He did refer, however, to Harrison's discoveries of 'paleoliths' on the Chalk Plateau of Kent, but confused these with the well-known river-drift implements.

THE EOLITHIC PERIOD

When Thomsen published his relative chronology for prehistoric times in 1836,

[1] Address of the vice-president and chairman of Section H—Anthropology—at the New York meeting of the American Association for the Advancement of Science.

[2] Vice-presidential address, *Proc. A. A. A. S.*, 1884, XXXIII., 579.

[3] Vice-presidential address, *Proc. A. A. A. S.*, 1899, XLVIII., 309.

the only stone age known was that which is now called the neolithic period. Boucher de Perthes's first discovery of paleoliths in the river drift of the valley of the Somme came just two years later. These river-drift implements, however, were not accepted until after (Sir) Joseph Prestwich's visit to Abbeville in 1859.

Is there a stone industry antedating the paleolithic? The answer depends in a measure upon the definition of the term. The Chellean *coup de poing* is quite generally looked upon as representing the oldest paleolithic industry. As to its position in the geological scale opinions differ. Piette and de Mortillet placed it in the Lower Quaternary. According to the more recent classifications of Rutot and Boule, the Chellean belongs to the Middle Quaternary. If the latter view is correct, then a pre-Chellean industry need not necessarily be of Tertiary age. The presence of artifacts in the Lower Quaternary should not be surprising even to the most sceptical. This is particularly true in view of the fact that the well-known almond-shaped implement represents an advanced stage in the art of chipping flint. While Professor M. Boule does not doubt that industrial remains may exist in the Lower Quaternary and even in the Tertiary, he denies that they have as yet been discovered. In his own words as a paleontologist he has a firm faith in the existence of Tertiary man, traces of whom he believes will some day be found. On the other hand, Dr. A. Rutot accepts as man's handiwork the rudely chipped specimens not only from the Lower Quaternary, but also from the Pliocene and Upper Miocene.

The first serious claim for the existence of a Tertiary industry was made by the Abbé Bourgeois in 1867. The subject at once attracted considerable attention; but after a lively discussion that lasted for five or six years it was relegated to the background. The specimens that Bourgeois found in the Upper Oligocene at Thenay are not at present accepted as artifacts.

Carlos Ribeiro's discovery of chipped flints in the Upper Miocene and Lower Pliocene at Otta and other localities near Lisbon was announced in 1871. An account of Delgado's researches at Otta was published in 1889. Professor Verworn,[4] who recently visited this locality, is of the opinion that the deposits there have been so disturbed as to make the age of the artifacts doubtful. They may be paleolithic and even neolithic.

The problem is simpler at the classic stations near Aurillac (Cantal). The best known of these are at Puy-Courny and Puy-Boudieu. Here the deposit in question is of Upper Miocene age, fossiliferous and undisturbed. It is covered by a bed of andesitic tufa that attains in places a thickness of from sixty to one hundred meters. There is no doubt as to the geological age of the chipped flints. As to the nature of the chipping, however, opinions differ.

When attention was called to the first specimens discovered by Rames in 1877, such well-known authorities as de Mortillet, Cartailhac, Chantre, de Quatrefages and Capellini declared that if these flints had been found in Quaternary deposits, no one would hesitate to regard them as artifacts. The Cantal industry has been carefully studied in more recent years by Capitan, Rutot, Courty, Klaatsch and Verworn, all of whom have decided in favor of its genuineness.

The revival of interest in a pre-paleolithic industry in England began when Mr. Benjamin Harrison, of Ightham, Kent, who had been collecting paleoliths from the

[4] Max Verworn, 'Archäolithische und paläolithische Reisestudien in Frankreich und Portugal,' *Zeit. für Ethnol.*, 1906, S. 611.

river drift of the neighborhood for years, extended his field of search in 1885 to include the summit of that portion of the chalk plateau which lies between the valley of the Darent on the west and that of the Medway on the east. Here at heights of from four hundred to seven hundred feet above the sea, he discovered flints supposed to have been chipped by the hand of man.

As Harrison's collection grew it was submitted to Sir Joseph Prestwich, whose country-seat was at Shoreham in the Darent Valley near by. Thirty years earlier Prestwich had confirmed the accuracy of Boucher de Perthes's discoveries in the valley of the Somme. Who could be better fitted than he to answer the questions as to the age of the specimens and of the southern drift in which they occur, as well as to the character of the chipping. According to Prestwich the rudely chipped flints are artifacts and are as old as the southern drift. They are both older than the northern drift or boulder clay, and hence preglacial. Rutot places them in the Middle Pliocene. The southern drift (with implements) was transported across the chalk escarpment and the chalk plain into the Thames Valley along lines independent of the present drainage; the patches that are now left on the highest points (eight hundred and sixty-four feet at Titsey hill west of the Darent Valley), marking what were then the valleys.

Mr. J. Allen Brown,[5] in discussing the specimens found on the North Downs by Harrison, was the first to propose the term 'eolithic,' now so much in evidence. Two years later, G. de Mortillet made use of the term in his 'Classification palethnologique,'[6] applying it to the Tertiary only. Dr. Rutot[7] does not limit the eolithic period to the Tertiary. In his classification, as previously stated, the early phases of the Quaternary are also eolithic, the well-known hache type (Chellean) not appearing until the second advance of the ice.

Eoliths are by no means confined to Kent. They have been found by Shrubsole in Berkshire; by Blackmore, Bullen and others near Salisbury, Wilts; at Dewlish in Dorset; also in Surrey, Hampshire, the southern part of Essex and Norfolk. Mr. Percival A. B. Martin has found eoliths at a number of places on the South Downs in the neighborhood of Eastbourne and Beachy Head.

Are eoliths artifacts? This is the fateful question. Their geological age is of no consequence if they are only natural forms and have never been used by man or his precursor. The first flakes to be utilized were in all probability natural forms. It is not likely that eolithic man knew how to obtain the raw material from the chalk. He depended on picking up from the drift flakes of approximately the shape and size needed. A sharp edge was utilized once, twice, or until it became dulled, and was then cast aside. If an angular piece did not admit of being comfortably grasped in the hand, the troublesome corners were removed. Such conclusions as these are forced upon one after careful examination of a series of the specimens in question. Would the same conclusions be so irresistible if these objects were merely nature's playthings? Many may even be grouped according to more or less definite patterns. Two of these deserve special mention, viz., the small crescent-shaped scrapers comparable to the spoke-shave, and the double scrapers with an intervening point between the two scraping edges. Sometimes two margins are worked, but on opposite sides. That is to say, after chipping one of the margins, instead of rotating the specimen

[5] *Jour. Anthr. Inst.*, March 8, 1892, XII., 93–94.

[6] *Bull. Soc. belge de géol., de paleon. et d'hydrol.*, Bruxelles, 1903, XVII., 425.

[7] *Bull. Soc. d'Anthr. de Paris*, 1894, p. 616.

until the adjacent margin comes into play, it was reversed.

Belgian archeologists were among the first contributors to our knowledge of a pre-Chellean industry. The discoveries by Neyrinckx in the railway cut at Mesvin, between Mons and Harmignies, date from 1868. M. Emile Delvaux later took up the work at Mesvin, where he succeeded in determining the presence of a rude industry antedating the paleolithic, to which he gave the name Mesvinian. During the past twenty years, Belgium's most indefatigable worker in the prehistoric field has been Dr. Rutot, his studies being confined chiefly to the Quaternary deposits.

The river valleys of Belgium are often marked by three terraces: the upper terrace, of Pliocene age, about ninety meters above the present water-level; the middle terrace, at an elevation of from twenty-five to sixty-five meters, and the lower terrace, a little above high-water-level, both of Quaternary age. The Quaternary may be divided into five series of deposits. Beginning with the oldest, these are: (1) Mosean, (2) Campinian, (3) Hesbayan, (4) Brabantian, (5) Flandrian. These deposits have been carefully examined by Rutot in quest of industrial remains.

With the exception of the Brabantian, which is above the eolithic zone, all five divisions of the Quaternary are represented in section in the exploitation Helin at Spiennes, near Mons, phosphate works now owned by the Société de Saint-Gobain. All of the Quaternary eolithic epochs are likewise represented here with the exception of the oldest, the Reutelian. Rutot found that the three separated industry-bearing Campinian layers each furnished one of the several elements composing the industry previously found elsewhere in disturbed Campinian deposits. In the lowest of the three, there were not only eoliths of Mesvinian age, but also rude implements roughly amygdaloid in shape, selected flint nodules only slightly chipped to a semblance of the hache type, or poniard. All the requirements of a transition industry between the Mesvinian (eolithic) and the Chellean (paleolithic) are therefore satisfied. The middle layer furnished examples of the classic *coup de poing;* and in the uppermost layer there were specimens of the hache type, carefully chipped on both sides until the margins presented almost a straight line as opposed to the zigzag margin of the Chellean implement—in other words, the so-called Acheulian industry of M. d'Ault du Mesnil. Rutot has proposed the name Strépyan for the industry of transition from the eolithic to the paleolithic because of the character and abundance of the specimens found at Strépy, on the right bank of the Haine, between Estinnes and Cronfestu.

Following Rutot's lead, many German investigators have taken up the search for a pre-paleolithic industry in northern Germany, particularly in the valleys of the Elbe and Spree and on the Island of Rügen. The chief contributors have been Professors H. Klaatsch, Eugene Bracht and Max Verworn and Drs. Hans Hahne, G. Schweinfurth,[8] Eduard Krause, *et al.*

For some years past, the spread of the eolithic propaganda has been so rapid as to cause dismay in the camp of its opponents. I spent the summer of 1903 in England and Belgium for the express purpose of studying the question at closer range. That summer's work formed the basis for a preliminary report[9] read at the St. Louis meeting of the American Association at the close of the same year, as well as for a more extended paper[10] pub-

[8] Schweinfurth's studies have been confined chiefly to Egypt.

[9] Science, 1904, p. 449.

[10] 'The Eolithic Problem—Evidences of a Rude Industry Antedating the Paleolithic,' *Amer. Anthropol.*, N. S., VII., 425–479.

lished in 1905. Before the latter was out of press there appeared an article by Professor Boule,[11] intended as a severe blow to the genuineness of eoliths. He had been trying for twenty years to stem the rising tide in favor of a pre-paleolithic industry and was beginning to think of instituting experiments in the hope of throwing light on the origin of eoliths, when M. A. Laville, preparator at the École des Mines, Paris, found an experiment station already in working order and turning out 'eoliths' daily by the hundreds. It was a cement factory on the left bank of the Seine, two kilometers southeast of Mantes, near Paris.

In extracting the chalk from the quarry most of the flint nodules are cast aside. Some, however, pass unnoticed by the workmen and are carried with the chalk to the factory. This, together with a certain amount of clay, is emptied into circular basins (délayeurs) or diluters. These vats have a diameter of about five meters and a depth of 1.4 meters. The water is supplied by means of conduits and finally escapes through lateral sieves, carrying with it the mixture of chalk and clay, both highly pulverized.

Each circular vat is provided with a horizontal wheel, the spokes of which are armed with cast-iron teeth that reach to within two tenths meter of the bottom, the wheel itself being just above the surface of the mixture. This wheel, with a diameter of five meters and making sixteen revolutions a minute, attains a velocity at the circumference of about four meters a second.

In this whirlpool of moving water, chalk, clay and iron teeth, are also the flint nodules that escaped the notice of the quarrymen. These nodules, therefore, receive thousands of knocks, some mutual, some from the iron teeth, until at the end of a period of twenty-nine hours the machinery is stopped and they are removed. They are then washed and piled up to await their ultimate use as a by-product. It was in one of these piles that M. Laville's[12] discovery was made. Later he visited the place in company with MM. Boule, E. Cartailhac and H. Obermaier.

According to Boule, the flints that have passed through the machine have all the characters of the ancient river gravels. Most of them have become rounded pebbles. Many, however, are chipped in a manner to resemble a true artifact. He and his companions were able in a few minutes to make a 'superb collection, including the most characteristic forms of eoliths, hammer-stones, scrapers, spoke-shaves,' etc. His article is illustrated by half-tone figures, which, however, are of very little use to the reader. Photographs of eoliths are practically useless; faithfully executed line drawings are little better; a view of the objects themselves is absolutely necessary before passing judgment on their origin.

Professor Boule does not pretend that all eoliths have a natural origin more or less analogous to those made by machinery. He does claim 'that it is often impossible to distinguish between intentional rudimentary chipping and that due to natural causes.' In his opinion, the artificial dynamics of the cement factory are comparable in every respect to the dynamic action of a natural torrent.

Nothing is really gained even by proving the impossibility of distinguishing between man's work and chipping due to natural causes. If the argument is worth anything it will admit of a still wider application because of the fact that it is admittedly impossible to distinguish between certain true eoliths and some artifacts of the paleolithic and neolithic periods. Things that

[11] "L'Origine des éolithes,' *L'Anthropologie*, t. XVI.

[12] 'Feuille des jeunes naturalistes,' 1905, p. 119.

are equal to the same thing are equal to each other. By substituting, therefore, one arrives at the identity between stream-made and machine-made eoliths, on the one hand, and recognized artifacts, on the other. This does not prove the non-existence of true eoliths any more than it does that of the paleolithic or neolithic artifacts.

If streams at flood ever produced eoliths it is more than probable that they may still be doing so. While keeping one eye on the chalk-mill at Mantes might it not be well to keep the other on the Seine that flows near by? A few Seine-made eoliths would certainly be more convincing than those turned out at the factory.

It has not been my good fortune to see one of those cement factories at work. It is evident from the available literature on the subject that considerable prejudice has entered into the controversy. MM. Laville, Boule, *et al.*, were evidently seeking for what they claim to have found at Mantes. On the other hand, it was extremely unfortunate that certain believers in an eolithic industry were refused admittance into the Mantes establishment. A selected series, however, from Mantes, which was sent in 1905 to the Salzburg meeting of the German Anthropological Association by Herr Obermaier, was later placed at the disposal of Professor Verworn and Dr. Hahne. Verworn compared them with his collection of eoliths from Cantal, while Hahne compared the Mantes specimens with a similar series from a chalk-mill on the Island of Rügen, and the eoliths from Belgium sent to him by Rutot.

Dr. Rutot has sought to match his superb collection of eoliths in the Royal Museum of Natural History, Brussels, with specimens from the chalk-mills of Belgium, but in vain. Professor Verworn and Dr. Hahne have been no more successful in their comparative studies. All three agree in their general conclusions as to the radical differences between the true and the false eoliths; also that the action of the mill is hardly comparable with that of the natural streams of the regions in question except in one particular, viz., both tend in time to make pebbles of the flints that are offered to them.

According to Professor Verworn,[13] a fundamental difference exists between the eoliths he found at Puy-Boudieu and the pseudo-eoliths from Mantes. The corners and edges of the latter are worn, while those of the Cantal eoliths are not. It has been suggested that the chipping on the specimens from Puy-Boudieu may be due to pressure of the overlying beds. Such a result might be possible where unstable beds contained a sufficient quantity of flint nodules and chips pressing against each other. At Puy-Boudieu, however, the chipped flints are not resting against each other. They are separated by masses of tufa, loam and sand.

After a careful comparison of machine-made eoliths from both Mantes and Sassnitz with the Mafflean and Mesvinian industry from Belgium, Dr. Hahne's[14] conclusions are as follows: (1) The chalk-mill flints are all scratched and otherwise marked by the iron teeth of the mill. (2) The sides of all the larger pieces are bedecked with scars from blows that were not properly placed to remove a flake. (3) Almost every piece shows more or less of the original chalky crust of the nodule. (4) Anything like a systematic chipping of an edge or margin is never found except for very short stretches where one would expect it to be carried along the entire margin. This is quite different from the long retouched margins of most eoliths. (5) The same edge is often rechipped first on one side and then on the other absolutely without

[13] *Op. cit.*, p. 620.

[14] 'Über die Beziehungen der Kreidemühlen zur Eolithenfrage,' *Zeit, für Ethnol.*, 1905, S. 1024.

meaning or purpose. The 'reverse working' of true eoliths is quite another thing. (6) In the mill product coarse chipping alternates with fine retouches along the same margin, while on the eolith there is a regularity and orderly sequence of chipping. (7) The repeated rechipping of the same edge, while others are left untouched, does not occur in machine-made eoliths. (8) The chief difference is between the haphazard and meaningless, on the one hand, and the purposeful, on the other.

The most prominent and easily breakable parts suffer most in passing through the mill. They are often retained intact, or only slightly altered, on the eolith to serve as a hand-hold, and there is a logical relationship between the worked and unworked portions.

The eolithic problem in northern Germany is even more difficult of solution than that of chalk-mill 'eoliths.' Dr. Klaatsch, who had previously made a study of eoliths in France and Belgium, was among the first to find so-called eoliths in fluvio-glacial deposits in the valley of the Spree. His discoveries were supplemented by Dr. Hahne's in the valley of the Elbe. At this latitude, the deposits of the first glacial and first interglacial period, containing what appears to be an eolithic as well as a transition (Strépyan) industry, were very much disturbed by the second advance of the ice.

The result is that the pieces in question are so badly damaged as to obscure the evidence bearing on their genuineness. The German geologists and anthropologists are divided as to the proper interpretation to place upon these specimens. After going over the material with Dr. Hahne, Rutot is of the opinion that after all doubtful pieces are rejected there will remain enough to establish the existence of an eolithic industry in those regions.

Another subject studied in common by Rutot[15] and Hahne is that of shore-made eoliths, the locality being a favored stretch of coast on the island of Rügen in the Baltic Sea. The chalk cliff is surmounted by a moraine with large erratic blocks. When the seas run high, the large blocks and glacial till are thrown forward over the cliff. Masses of the fissured chalk are also loosened and fall to the foot of the cliff. With recurring high seas, broken nodules of flint come in contact with the erratic blocks and the production of pseudo-eoliths begins. If left, however, to their own fate they are finally reduced to sand. When rescued at the proper time, they resemble more or less the true eolith. They certainly form a more convincing argument in favor of the natural origin of all eoliths than do those from the chalk-mills. But they resemble the latter more than they do the genuine eolith, which, according to Rutot and Hahne, is still unaccounted for unless it represents the handiwork of man or his precursor.

The differences are not great enough to be detected by the untrained eye. They may be compared to the differences between hand-made and machine-made music. The untrained ear might not detect them without seeing the operator at work, but no such substitution could deceive an expert. It would be rather wide of the mark to conclude that, because pianos may be played by a pianola, they were never played by hand. Or if ever played by hand the result must necessarily be identical with that produced by the pianola.

The wide differences of opinion in the opposing camps can hardly be due to prejudice alone. Faulty or insufficient observation and incorrect interpretation doubtless play their part. Luckily, there is no disposition to drop the matter until the truth appears. At the International Congress of Anthropology and Prehistoric Archeol-

[15] 'Eolithes et pseudo-eolithes,' *Mém. de la Soc. d'anthrop. de Bruxelles*, 1906, t. XXV.

ogy held at Monaco, April 15 to 22, 1906, the chief subject of the second session was the pedigree of the eolith. According to *Nature*,[16] "a series of mill-modeled flint nodules was exhibited, among which there were certainly a number closely resembling many Prestwichian types, but conspicuous by their absence were the decidedly purposeful and rationally usable Kentian forms." On the other hand, Professor E. Ray Lankester "submitted that he had recently placed on exhibition in the British Museum a considerable series[17] of specimens selected from Prestwich's collection, all borer-like in form, too identical in shape and so rationally of obvious utility for any possibility of their being the result of fortuitous natural collisions."

As a further indication of the importance attaching to a correct solution of the problem and indirectly in recognition of the value of Rutot's contribution toward such a solution, the meeting of the German Anthropological Association for 1907 will be held in Cologne[18] in order that the members may visit the eolithic stations of Belgium and see the collections of the Brussels Museum.

THE ART OF THE CAVE-DWELLER

Passing now from Tertiary and Lower Quaternary eoliths and leaving out of account for the time being the important industry of the Chellean and Mousterian epochs, we come upon a most interesting chapter in the history of paleolithic man—a chapter to which many attractive pages have been added during the past ten years. It concerns the art of the cave-dweller.

One of the earliest enlightened searchers for prehistoric man in caverns was the Rev. J. MacEnery, a Roman Catholic priest, stationed at Torquay on the southern coast of England. As early as 1825 he found, in Kent's Cavern, flint implements definitely associated with the remains of the mammoth, rhinoceros and other extinct animals.

Then came, in 1833, the discoveries of Schmerling in the caverns about Liége, Belgium; but these also did not receive the attention they deserved, owing to the then all-powerful influence of Cuvier.

Following the appearance of Darwin's 'Origin of Species' and the acceptance of the river-drift implements as artifacts, both of which events occurred in 1859, cavern explorations received a new impetus. In Europe alone hundreds of paleolithic caverns have already been explored. About one third of these are situated in France. Some of the well-known localities outside of France are the regions about Namur and Liége, Belgium; Liguria in Italy; Moravia in Austria; and Schaafhausen in Switzerland.

To the student of the cave-dweller period, southern France is perhaps the most fruitful field in all Europe. Of this area Les Eyzies and its environs in the valley of the Vézère, department of Dordogne, is one of the chief centers. It was in 1862 that M. J. Charnet found in a shallow cave at Les Eyzies the first flint implements and breccia, with bones of the reindeer and other animals. He communicated the facts to Professor Eduard Lartet, of Paris, and Mr. Henry Christy, of London, who visited the place in August, 1863, making explorations simultaneously at Les Eyzies, Gorge d'Enfer and Laugerie-Haute.

This led, in 1865, to the plans for a great publication to be called 'Reliquiæ Aquitanicæ.' In importance, as well as in authorship, this is an international volume. The specimens described belong to the Christy collection of the British Museum

[16] June 28, 1906, p. 211.

[17] *Amer. Anthropol.* (N. S.), 1905, VII., 432, 433.

[18] It is proposed to make the Cologne Congress international. American anthropologists have been invited to take part.

and to the Museum of National Antiquities at Saint-Germain. The Lartet and Christy explorations were practically confined to the rock shelters and shallow caves. If these men had lived longer, they might have discovered the extensive subterranean caverns of the neighborhood, the walls of which are decorated with a remarkable series of frescoes and engravings.

The valley of the Vézère has been an attractive field for archeological excursions ever since the appearance of Lartet and Christy's classic work; and has come even more into favor since 1895, when the first discovery was made of engravings and frescoes on the walls of one of the caverns. Several other caverns have been explored during the past six years with similar results. In company with a small party of Frenchmen from Paris, members of the Société des Excursions Scientifiques, I visited the region during the summer of 1903.

Vézère collections have found their way practically into all the important museums of the world, but the British Museum (Bloomsbury), the Natural History Museum, Paris, and the Museum of National Antiquities at Saint-Germain-en-Laye contain the major part. These should all be visited before, as well as after, a trip to the Dordogne. The train can be taken direct from Paris (Gare d'Orléans) to Perigueux, the capital of the department of Dordogne, the site of ancient Vesuna of the Petrocorii and later a flourishing Roman town. Here one may stop with profit to see the ruins of a Roman amphitheater and tower, also the Musée de Périgord, rich in prehistoric relics of Dordogne, including the Vézère region.

From Perigueux it is less than two hours by train to Les Eyzies, the heart of the cave-dweller country, where one stops at the Auberge Berthoumeyrou, well and favorably known to a long line of pilgrims to this enchanted land of limpid streams, green valleys and lofty, picturesque escarpments.

The calcareous formation, cleft by the Vézère and its tributaries, is composed of Cretaceous beds approximately horizontal and of varying degrees of hardness; so that overhanging rocks often shelter horizontal galleries and niches. Again subterranean streams have left meandering caverns, some of them several hundred meters in length. These as well as the rock-shelters and open, shallow caves, formed through atmospheric agencies, were inhabited by early man. Some were enlarged or modified and occupied during the middle ages. At a safe height in the *roc de Tayac,* one such that withstood successive sieges in the fourteenth and fifteenth centuries is at present used as a restaurant and appropriately named 'au Paradis.'

The earlier explorations at Les Eyzies, Cro-Magnon, Gorge-d'Enfer, Laugerie-Basse, Laugerie-Haute, La Madeleine and Le Moustier are so well known that they are mentioned only in passing. After so long a series of important discoveries, it might well be supposed that the archeological possibilities of the region had been exhausted, yet some of the most important treasures still remained locked in the recesses of the less easily accessible and little known subterranean caverns which penetrate the hills to great depths. The entrances to these caverns are small and invisible from the valley below. Some, indeed, were completely stopped by hillside débris, leaving no outer trace of their existence. It is not strange that they escaped immediate notice. They were neglected until the early nineties, when Rivière removed some of the floor deposits in the cavern of Combarelles that yielded many flint implements, and especially fine bone needles. In 1895 he began work in similar deposits in the cavern of La Mouthe. One day, after penetrating to a considerable

depth, he and his companion, the son of Berthoumeyrou, the innkeeper, sat down to rest. In lighting a cigar, the extra light of the match added to the feeble candle light and placed at the proper angle revealed to one of them what had not been observed before—an engraving on the wall. The discovery was duly announced and marked the beginning of a new epoch in cavern explorations. Not that Rivière's discovery was the first of its kind; but that the two previous finds of a similar nature had not been accepted as authentic. These were in the cavern of Altamira, Province of Santander, Spain, explored in 1879 by Sautuola,[19] and of Chabot (Gard), explored prior to 1889 by Léopold Chiron. Then followed Rivière's discovery at La Mouthe and that of Daleau in 1896 in the cavern of Pair-non-Pair (Gironde).

The mural decorations at La Mouthe occur in four groups or panels. The first panel is about ninety-three meters from the entrance. The second, four meters farther on, is called the 'Hall of the Bison.' Seven animals are represented on an area 5.02 m. by 2.6 m. The third and fourth panels are one hundred and thirteen and one hundred and thirty meters, respectively, from the entrance.

In 1899, Rivière was so fortunate as to find a stone lamp in the floor deposits of this cavern at a point about seventeen meters from the entrance. The pick of the workman broke the lamp into four pieces, of which three were immediately recovered. Rivière and two of his men searched for the missing fragment an entire day, but without success. The shallow bowl contained some carbonized matter, an analysis of which led M. Berthelot, the chemist, to conclude that lard was used for lighting purposes. On the base, there is an engraving of a wild goat's head and horns. A figure exactly like this was found on the third mural panel already mentioned. This was the fourth lamp to be found in French caverns. The first and second were from the cavern of Monthier (Charente), and the third from the cavern of Coual (Lot). The necessities of men dwelling in dark caverns would be likely to lead to the invention of artificial light, which light made it possible for them to depict the frescoes and engravings on the walls of their abodes.

The past six years have witnessed a succession of remarkable discoveries by MM. Capitan, Breuil, Bourrinet and Peyrony, in the caverns of Combarelles, Font-de-Gaume, Bernifal and others.

The Combarelles cavern has a total length of two hundred and thirty-four meters, is from one to two meters wide, and high enough to admit of walking upright for most of the way. The engravings begin at a point about one hundred and eighteen meters from the entrance, and occupy both walls for a distance of one hundred meters. Some of the figures are deeply incised; others are mere scratches. In some, the effect is heightened by the application of a dark coloring matter (oxide of manganese). Portions of the walls are covered by a coating of stalactite thick enough in places completely to hide engravings; while in others the more deeply incised figures are still visible. On areas devoid of incrustations, the figures are fresh and distinct. The artist sometimes had recourse to champlevé; sometimes natural prominences were utilized to add relief to the figures. Of the one hundred and nine engravings of various animals on the walls at Combarelles there are some forty equine figures, occurring either singly or in groups, and fourteen of the mammoth. The mural engravings belong precisely to the same school of art as the relief and incised figures

[19] Don M. S. de Sautuola, 'Breves apuntes sobre algunos objetos prehistoricos de la provincia de Santander,' 1880, Libreria Murillo, Madrid.

from the floor deposits of the shallow caves and rock shelters, so well known through the works of the earlier investigators. This statement applies equally to all the caverns thus far explored.

The cavern of Bernifal was first explored in 1903. It was discovered by accident. The original entrance near the base of an escarpment is completely obstructed by earth and stones. The present artificial entrance is at a point where the ceiling of the cavern comes close to the surface of the wooded, sloping upland. The descent into the cavern is almost vertical, and made by means of an iron ladder about three meters long. There is a joint in the ladder, the upper portion of which may be inclined and locked so as to secure the interior against vandalism.[20] Within are three large chambers united by rather narrow corridors. The first is twenty-two meters long, with high ceiling and a maximum breadth of eight meters. The others are not quite so large. The beautiful stalactites overhead have been left undisturbed. Most of the engravings are to be found in the second chamber. They are cut rather deeply into the calcareous walls, and generally coated over with a thin, hard layer of stalactite. Twelve groups, numbering in all twenty-six figures, have been recognized. These include geometric, triangular signs, in addition to various animal figures—reindeer, mammoth, horse, bison and antelope. Some are simply engraved, others are painted with red ochre and manganese. Many are probably wholly hidden beneath thick mural incrustations. Tectiform signs, the significance of which is unknown, were also met with at Combarelles and Font-de-Gaume.

The Font-de-Gaume frescoes and engravings were discovered in 1901 by Capitan and Breuil with the assistance of M. Peyrony, the school principal of Lez Eyzies. The entrance is some twenty meters above the valley and near the top of the escarpment.

A passage about sixty-five meters long, and much restricted in places, leads to an ample gallery forty meters in length, two to three in breadth and five to six in height. A majority of the paintings, and Font-de-Gaume is especially rich in paintings, occur on the walls of this gallery and in a little side chamber farther on. The latter contains thirteen remarkable figures, in color, of the bison and a group of reindeer. The coloring matter was red ochre and manganese, either mixed so as to give various intermediate shades or used separately. Both these materials are found on top of the neighboring plateaus. The dimensions of the figures vary from 2.70 m. down to .20 m. Some are on regular surfaces, while others include natural prominences in such a way as to give the effect of relief. They are veritable frescoes, the whole figure often being covered with paint. Engraving and fresco are usually associated in the same figure. The coloring matter was, in some cases, applied after the engraving; while in others the process was reversed. Again some figures are a piece-work of engraving and fresco. Some are engraved only. In certain cases the outlines of the animal are simply traced by a single stroke of the brush or pencil, usually in black. Where the contours are filled in, various tints from black to red are usually employed. The outlines are seldom marred by blotches or evidences of an uncertain stroke.

Of the more than eighty figures described already from Font-de-Gaume, forty-nine represent the bison, four the reindeer, four the horse, three the antelope, two the mammoth, one the stag, one *Felis leo*, one *Rhi-*

[20] Most of the prehistoric monuments of France are now the property of the government and are protected by the enactment and enforcement of wise laws.

noceros tichorhinus, six various signs. A number have not yet been determined.

In their various explorations MM. Capitan, Breuil and Peyrony have collected about a hundred drawings of the mammoth. Those of the bison, horse and reindeer are also numerous. On the other hand, representations of *Ursus, Felis* and *Rhinoceros* are rare. The engraving of *Ursus spelæus* on a piece of schist found in the floor deposits of the cavern of Massat (Ariège) has been known since 1867. A similar figure is to be seen on the cavern walls of Combarelles. An engraving of *Felis* on a pebble from the cavern of Gourden (Haute-Garonne) was recently published by Piette. Two mural engravings of *Felis* are known; one at Combarelles and the other at Font-de-Gaume. In the latter the entire animal is represented, being characterized by the form of the head, the general aspect of the body, the long, lifted tail and short paws. The animal is probably *Felis leo,* var. *spelæa,* since it is figured somewhat larger than are the four horses forming part of the same group or picture.

One of the most interesting animal representations on the cavern walls of Dordogne is a color drawing of *Rhinoceros tichorhinus,* found at Font-de-Gaume near the group that included an engraving of the cave lion. The figure is not only complete, but also exact. The two horns are faithfully indicated, the anterior notably longer and larger than the posterior. The only other representation of the woolly rhinoceros is an indifferent engraving on a piece of stone found in the cavern of Gourdan and recently published by Piette.

The cavern of Font-de-Gaume opens on a narrow valley tributary to that of the Beune and near their junction. The well-known rock shelter of Les Eyzies lies across the valley of the Beune. It is visible from Font-de-Gaume, appearing like a black spot on the face of the great escarpment, and only eight hundred meters distant. M. Peyrony[21] suggests that the two prehistoric communities may have been closely united. His recent researches at Les Eyzies tend to confirm this view.

The shallow cave of Les Eyzies, overlooking the Beune near its junction with the Vézère, opens on a sort of natural platform about thirty-five meters above the bed of the stream. The opening of the cave is wide and high enough to admit the light to its greatest depth, which is twelve meters. The geatest width is sixteen meters. It has a southern exposure; is dry and habitable. Font-de-Gaume was never a place of residence, as is indicated by the absence of floor deposits. About the only objects found there are a few broken gravers with edges dulled in executing the wall engravings, a few pieces of ochre and manganese and one handsome ochre pencil. Why should the artists make residence of a dark subterranean cavern, when by going a short distance they could have an ample shallow cave or rock shelter facing the south and warmed and lighted by the sun? Such a shelter is Les Eyzies, and the enormous quantities of refuse taken from its floor at various periods testify to its use as a place of habitation by generation after generation.

The rock shelter of Les Eyzies has furnished unusually large quantities of ochre of various tints. Most of the pieces have been scraped to produce a colored powder which was mixed with grease or some liquid, thus forming a paint. In order to pulverize and thoroughly mix the coloring matter, mortars were used. An interesting series of these mortars from Les Eyzies

[21] Le Dr. Capitan, l'Abbé Breuil et Peyrony, 'Nouvelles observations sur la grotte des Eyzies et ses relations avec celles de Font-de-Gaume,' *Compte rendu, Congrès préh. de France,* 1905, p. 137.

forms a part of the famous Christy collection in the British Museum. Very few mortars have been found in neighboring stations. Besides, ochre pencils exactly like the one from Font-de-Gaume have been found in the rock shelter of Les Eyzies. Sometimes a flat piece of ochre is cut in the form of a triangle, each angle serving in turn as a pencil point. Some of these pencils are perforated to be suspended, and might well be supposed to form a part of the outfit of the artists who drew in color figures such as that of the two-horned rhinoceros previously mentioned.

It may be that the artists who made their home at Les Eyzies decorated its walls also. Exposure would have obliterated these decorations long ago. Lucky it was for present-day lovers of art and archeology that their troglodyte forebears had the good sense to seek at Font-de-Gaume a more permanent gallery for their masterpieces.

In addition to the four caverns with wall engravings and paintings in the Vézère valley group, one other is now being explored in the Dordogne, viz., the cavern of La Mairie et Teyjat. This large cavern is only two hundred meters distant from the rock shelter of Mège, discovered in 1903 by M. Bourrinet. In the cavern of La Mairie the floor deposits may be separated into two industry-bearing layers. The upper one of these contains the same industry as the single layer in the adjacent rock shelter of Mège, except that the latter has furnished archeological material in greater quantities than were found in the deposits of the cavern. The relative positions of the engravings on the cavern walls and the upper layer of floor deposits prove that both belong to the same epoch (Magdalenian). It is also interesting to note that while bones of the reindeer abound in the rock shelter of Mège, representations of this animal predominate among the mural engravings in La Mairie cavern.

Besides the cavern of Chabot, at Aíguèze and of Pair-non-Pair, already mentioned, other decorated French caverns explored to date are: Le Figuier (Ardèche) across the river from Chabot, La Grèze and Marsoulas (Haute-Garonne).

Of caverns with paleolithic mural decorations outside of France, thus far reported, one is in Italy and four are in Spain. The most important cavern in the Spanish group is that of Altamira in the north coast province of Santander, previously mentioned as being the one in which the discovery of mural figures first took place. The genuineness of these figures would have continued to remain in doubt had it not been for similar subsequent discoveries elsewhere.

M. Émile Cartailhac and the Abbé H. Breuil have recently studied with great care the wall paintings and engravings at Altamira. The cavern is a series of large chambers connected by passageways. There is no evidence of its having been occupied by either man or beast since the close of the Quaternary, at which time the entrance was completely closed by a fall of earth and stones.

A second, recent fall has afforded a new opening to the cavern, reached by clambering over the débris that closed the original entrance. The first chamber is divided by means of a mass of fallen stones. The one on the left is forty meters long by twenty meters wide. The one on the right is a sort of corridor connecting with other chambers. Industrial remains of the floor deposits are confined to the entry and the chamber on the left. There is evidence that the cave bear had occupied the cavern before man took possession. Figures, engraved or painted, are found on the walls of every part of the cavern, especially on

the ceiling of the chamber on the left near the entrance, where the frescoes are remarkable for their beauty, size and good preservation—a sort of Sistine chapel representing the *chef d'œuvre* of perhaps more than one Michael Angelo of that far-off time.

These works of art represent a variety of technique. Some are simple line engravings. Others are more deeply incised. But the engravings are not so numerous as the figures represented in color. Many are done in a single color, either red or black. The most remarkable are the polychrome frescoes similar to those of Font-de-Gaume, already described.

The figures are not all animal representations. Many are signs, the significance of which is not known. They do not belong to a single epoch. The superposition of figures, each in a different technique, studied in connection with the relative state of preservation of the various figures has furnished a key to the order of succession. The same succession is traceable in the caverns of France, so that the Abbé Breuil and his colleagues: MM. Cartailhac, Capitan, Peyrony and Bourrinet, have been able to distinguish four distinct phases[22] in the evolution of mural painting and engraving, all of them being represented in the cavern of Altamira.

The *first phase* includes deeply incised figures representing the animal in absolute profile, *i. e.*, with a single forefoot and a single hind foot, the outlines being rude and not well proportioned, and details, such as hoofs and hair, not indicated. The figure of a bison in the corridor on the right is a specimen of this oldest class of wall decoration. Other examples are found at La Grèze, Chabot and Pair-non-Pair.

The paintings of this stage are also in outline, the color being black or red and drawn with a crayon, there being absolutely no effort at modeling. The horse drawn in black on the ceiling of the left chamber is an illustration. Others may be seen at Marsoulas, Font-de-Gaume, La Mouthe, Combarelles and Bernifal.

The incised figures of the *second phase* remain deep and broad; but the outlines are more lifelike although often ill-proportioned. All four legs are often represented, the distal ones being almost completely hidden by those nearest the beholder. The hoofs are sometimes represented with great care. As the incisions become less deep they also gain in neatness. In places the effect of bas-relief is given by means of champlevé. The more hairy portions are indicated by incised lines. Engravings of this stage are not numerous at Altamira. They are seen to better advantage at La Mouthe, Font-de-Gaume, Bernifal and above all at Combarelles.

The paintings of this phase evince the first attempts at modeling by shading at various points. Engraving is often combined with the painting. The use of color continues to develop until one arrives at a monochrome silhouette usually in black. The contours are often heightened by engraving. The second phase is represented not only at Altamira, but also at Marsoulas, Combarelles, Font-de-Gaume and La Mouthe.

The engravings of the *third phase* are generally of small dimensions. Many of these are admirable in their execution, as, for example, the bison in the terminal corridor. The entire mural decorations in the cavern of La Mairie at Teyjat are in this style, as are a number from Font-de-Gaume and Marsoulas.

In the domain of painting, the third phase is represented by an excessive use of color, producing a flat effect, thus destroy-

[22] A fifth and closing phase is discernible at Marsoulas, resembling somewhat the work on the painted pebbles of Mas d'Azil.

ing the modeling that was such an attractive feature of the preceding stage. At Altamira the color employed is red and the drawing is deplorable. As a rule these examples are not well preserved. Those from Marsoulas, in either black or red, are not much better. The best work of this phase is to be seen at Font-de-Gaume and is executed in black or brown. It is often combined with engraving of a high order, done before the color was applied.

In the *fourth phase* the engravings lose their importance. The lines are broken and difficult to follow. The small figures of the mammoth at Font-de-Gaume and of the bison at Marsoulas show this tendency to emphasize detail at the expense of the ensemble.

Paleolithic painting reached its zenith in the fourth phase. The outlines are drawn in black, as are the eyes, horns, mane and hoofs. The modeling is done with various shades produced by the mixing of yellow, red and black. Engraving always accompanies the fresco, serving to emphasize the details. These polychrome figures are seen at their best on the ceiling of the left chamber near the entrance; also at Marsoulas and Font-de-Gaume.

Shortly before his death, M. P. Jamin, a well-known Parisian artist, exhibited in the Paris Salon of 1903 a large oil painting inspired by the discovery of these polychrome frescoes. This canvas also formed part of the French art exhibit at the Louisiana Purchase Exposition, St. Louis, in 1904. It represents the cave-dweller artist in the little side chamber of Font-de-Gaume at work on one of the thirteen figures of the bison while members of his family look on and applaud. In a panel above his head is the unfinished group of reindeer. It has long been the custom for artists to copy the old masters. M. Jamin has rendered a valuable service to both art and archeology by introducing the modern French school of painters to the earliest school of art developed on what is now French soil. The ages of Phidias and of the Italian Renaissance, viewed in the light of their antecedents, are wonderful manifestations; but not more wonderful than that of the Vézère troglodyte, a contemporary of the mammoth and rhinoceros, the bison and the reindeer.

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AMERICAN SOCIETY OF BIOLOGICAL CHEMISTS

FOR several years the biological chemists of this country have been considering the advisability of organizing a national biochemical society. The growth of the Society of Physiological Chemists (New York City), which was founded in 1899, the development of the biochemical section of the American Chemical Society, which was organized in 1905, the increasing number of chemical papers on the programs of the American Physiological Society, and the great success of the recently established *Journal of Biological Chemistry,* were among the influences that stimulated thoughts of a national organization of biochemical workers.

At the suggestion of Professor John J. Abel, a meeting for the purpose of effecting the establishment of such a society was held in New York City, at the headquarters of the American Association for the Advancement of Science (Hotel Belmont), on the afternoon of December 26, 1906. About seventy-five American biological chemists had been invited by Professor Abel to attend the meeting, but many were unable to go to New York at the time stated. There were few, however, who did not heartily favor the project. Of those who had been invited to attend the meeting the following were present:

John J. Abel, Carl L. Alsberg, Samuel Amberg, Silas P. Beebe, Russell H. Chittenden, Otto Folin, Nellis B. Foster, C. Stuart Gager, William J. Gies, Robert A. Hatcher, Reid Hunt, Holmes C. Jackson, Walter Jones, Waldemar Koch, Phoebus A. Levene, Arthur S. Loevenhart, John A. Mandel, John Marshall, Gustave M. Meyer, Thomas B. Osborne, Raymond H. Pond, Alfred N. Richards, Herbert M. Richards, William Salant, Philip A. Shaffer, Herbert E. Smith, Frank P. Underhill, George B. Wallace, Charles G. L. Wolf.

The meeting was formally addressed by Professor Abel, who, in urging the desirability and timeliness of immediate organization of biological chemists, made the following remarks:

"I take the liberty of rehearsing briefly the reasons for which this meeting has been called. * * * We have become convinced that there is need in this country for an organization which shall further the interests and foster the growth of biological chemistry. Biological chemists at present are affiliated with widely differing societies and come little in contact with the great body of men who are interested in biochemical work. Whether we as chemists have as our field of work the physiological chemistry of our medical schools or deal with the chemical problems of botany, zoology, pathology, pharmacology or medicine, we all have one common meeting-ground, and that is chemistry as applied to animal or vegetable structures, living or dead. As distinguished from the work of pure chemists, organic or inorganic, our efforts are directed towards throwing light on the life processes and functions of living structures, with the help of chemical and physico-chemical methods.

"Now, it will be granted, I think, that scattered and divided forces cannot develop that coordination of effort that is desirable when many workers have one great interest in common. In such a case, organization is beneficial. It encourages research, it furnishes the mechanism for competent criticism and helpful discussion; and lastly, the very fact that we have felt impelled to organize will make it evident to faculties of science and medicine and to scientific and medical societies that a great and growing department of research demands its fitting place in the general scheme of higher education.

"I come now to the question of an academic career in biological chemistry. You have probably all, at one time or another, been asked to recommend some young man for a teaching position in physiological chemistry. The authorities in question want a man who has had a first class training in organic, inorganic and physical chemistry and biology, has had some experience in teaching physiological chemistry, has an agreeable personality, is a fascinating lecturer, and a promising if not already fruitful investigator. For such a rare combination of natural endowment and acquired culture, there is offered a salary ranging from $800 to $1,500, the title of assistant or instructor, with guarded hints as to promotion at some uncertain date and still more non-committal statements as to a possible rise in salary.

"Biochemical research is quite the thing to-day. Every species of laboratory, clinical, bacteriological, hygienic, pathological, pharmacological, wants a chemist. All these laboratories no doubt afford fine opportunity to the young chemist for training in the broad field of biological chemistry. But what of his future? Is it as promising as it should be?

"This state of affairs is largely our own fault. We attend only the meetings of societies of other specialists for fear we shall lose something that lies on the border line between their territory and ours. These other specialists have their house in

order, organization has done its invaluable service for them, and the result is that every worker knows his fellows, each knows where to turn for advice and sympathy; each member, no matter how remotely placed or how depressing his immediate environment, has the courage and enthusiasm in his work which comes from being connected with those who have the profound conviction that their branch is one of prime importance and dignity.

"I believe in special societies for specialists and I have no fear of the so-called narrowing influence of specialization. I feel rather that any possible danger in that direction is more than offset by the stimulus to go deeply into our subject which comes from association with those of like interests. Chemistry, the fundamental science that must always guide our work, offers unlimited opportunity for broadening the mind.

"It is my firm conviction that a national society of biological chemists should be organized at once. There are in this country, as near as I can ascertain, about one hundred active workers in this field, using the term in its widest sense. A very small minority of those with whom I have corresponded are undecided as to the wisdom of forming such a society, but are willing to accept the action of the majority. Some of these, again, have raised the question as to the advisability of asking the Physiological Society to give us a separate chemical section.

"Many of us have given careful thought to this proposition, but have decided that it will be best to have an independent organization. I have already outlined some of the advantages that would follow on organization, and I can only repeat that I believe these advantages would be greater if in name and fact the organization is independent. I believe that we can have a society on broader lines than is possible to a mere section. We wish to draw into our society the biological chemists of all departments of biology including those organic and physical chemists who take a lively interest in our subject, but who would perhaps not care to join a physiological society. In fact, since a large number of our proposed membership are primarily chemists rather than physiologists, we should be marching under a wrong banner, no matter how great the freedom granted by the parent society.

"This desire for, or prejudice, if you will, in favor of, entire independence in name and action, would equally forbid our organization as a section under the American Chemical Society. While recognizing that the various branches of science are mutually dependent and constantly receiving help from each other we still contend that special devotion in each individual branch alone insures success. In other words, we should stand for *independence* with *interdependence.*"

At the conclusion of Professor Abel's remarks, which were warmly applauded, general discussion ensued. Hearty approval of his proposal was evidenced, and it was agreed to organize at once the society that Professor Abel suggested. The undersigned thereupon proposed a few written articles of agreement on which a permanent organization could be based. These articles of agreement were adopted and will govern the society until the formal ratification of constitution.

On motion by Professor Abel, Professor Chittenden was elected president of the society. In accepting the presidency, Professor Chittenden expressed appreciation of the reasons which led to the establishment of the society, and declared his hearty interest in its future growth and success.

The following council and officers were elected: John J. Abel (*vice-president*), R. H. Chittenden (*president*), Otto Folin,

William J. Gies (*secretary*), Walter Jones, Waldemar Koch, John Marshall, Lafayette B. Mendel (*treasurer*) and Thomas B. Osborne.

No plans have been made for a meeting before next December, although the council was authorized to use its discretion in this and all other matters affecting the welfare of the society.

WILLIAM J. GIES,
Secretary

SCIENTIFIC BOOKS

The Bird: Its Form and Function. By C. WILLIAM BEEBE, Curator of Birds, New York Zoological Park. New York, Henry Holt & Co. 1906. 8vo. Pp. 496, with over 370 illustrations.

This book is 'intended as an untechnical study of the bird in the abstract' and tells of the structure and characteristics of birds, dwelling especially upon the adaptations of the various organs to their uses, and their bearing on the relationships and past history of birds. It thus covers ground that has been but little worked, for while there are books a many on the anatomy of birds, these, with the exception of Headley's 'Structure and Life of Birds,' are purely descriptive and fail to show the relations of a bird's structure to its surroundings and mode of life. Here we are told why a beak, a foot, a wing, is of a given shape, what rôle it plays in a bird's daily life, or, if its present use is not obvious, what hint it gives of a bird's past history when the part now useless was all-important.

The first chapter, devoted to the ancestors of birds, of necessity recapitulates what is already known—what we do *not* know will fill volumes still to be written. We would, in passing, dissent from the statement that Archæopteryx 'frequently walked or ran on all fours,' and if Mr. Beebe will make a figure of the animal in such a position he will doubtless appreciate the difficulties in the way. Next is a long chapter on 'Feathers,' including their origin, structure, development, arrangement and moult, and this is followed by a discussion of 'The Framework of a Bird,' the skull being given a chapter by itself. Much information as to color and color changes will be found under the caption 'The Body of a Bird,' where some good illustrations are given of the effects of food, light and moisture, one of the most striking being the very dark form of the white-throated sparrow, produced by exposure to moisture-laden air through two moults. So, part by part, the bird is considered in detail, the final chapter treating of 'The Bird in the Egg.' Under 'The Eggs of Birds' we learn of the eggs themselves and of the information that may be gathered from them when studied in connection with the habits of the bird that laid them. For "That which adds the greatest interest to anything is the '*why*' of it, and a vast collection of eggs, beautiful though they are, yet if ignorantly looked at is worse than useless. Why one bird lays twenty eggs and another but two; why one bird's eggs are white, another's of varied colors, we will never learn from blown museum specimens." It has been denied that oology is a science, but whether it is or is not depends on the individual and it is to be hoped that this chapter may afford fruitful suggestions for future work on the part of our younger ornithologists. The chapters on Wings and Beaks and Bills are among the best because Mr. Beebe, who is a keen observer, has here combined the results of his experience in the field, and of the opportunities offered by having many species of birds continually under his observation in the New York Zoological Park. In regard to beaks we are told that "The finding and securing of food being the most important problem birds have to solve for themselves, it is for these purposes, and especially the last mentioned, that we find bills most adapted. This is so universally the case that we may often judge accurately of the kind of food of a certain bird from a glance at its beak."

It is impossible that a book of this nature, where much information is crowded into a small space, should be entirely free from errors, and here and there slips occur. Thus we are told that *Amphioxus* has biconcave vertebræ, after having been correctly informed that the backbone is represented by a mere thread of gristle; that the moa was found in

Madagascar; and that the femur of a bird is short *because* the knee is concealed. There is also an occasional tendency to overestimate the size of birds; for example, it is stated that the South American condor sometimes has a spread of wing of fifteen feet. As a matter of fact this bird is slightly exceeded by the California vulture, whose greatest recorded spread is ten feet six inches, and it is an unusually large condor that measures even nine feet from tip to tip. We *believe* that the albatross sometimes exceeds twelve feet across the wings and *may* reach fourteen, but never measured one greater than ten. However, the above are but minor blemishes, the book abounds in information and represents a large amount of original work.

The illustrations, mostly from photographs taken by the author, are numerous, and, with rare exceptions, good. One of the exceptions is that on page 85, showing the shoulder girdle of a pigeon, and is not only taken from a diseased specimen, but fails to show the parts described. Some particularly good pictures are to be found in 'Heads and Necks' and 'Wings,' the young green heron and his *vis-à-vis*, the great white heron, being most excellent.

All in all, this volume of The American Nature Series is admirable.

F. A. L.

Einleitung in die Chemische Krystallographie. By P. GROTH. Pp. v + 80, 6 figures, 8vo, cloth, 4 marks. Leipzig, Wilhelm Engelmann. 1904. English translation by HUGH MARSHALL, 12mo, cloth, $1.25. New York, John Wiley & Son. 1906.

Chemische Krystallographie. By P. GROTH. In four volumes. Vol. I., pp. viii + 634. 389 figures, 8vo, cloth, 20 marks. Leipzig, Wilhelm Engelmann. 1906.

For nearly a score of years Professor Paul von Groth, of the University of Munich, has had in preparation this 'Chemische Krystallographie' which aims to include in systematic order trustworthy data of all crystallized chemical substances.

The introduction to this monumental work appeared as a separate publication under the title 'Einleitung in die Chemische Krystallographie' in 1904, and has since been translated into English. In the 'Einleitung' the recognized relations existing between the properties of crystals and their chemical constitution are explained in the light of modern ideas of crystal structure. In so doing Professor von Groth assumes a knowledge of physical crystallography and chemistry. In order, such topics as crystal structure and its varieties, polymorphism, morphotropy, isomorphism, and molecular compounds are discussed. In the chapter on morphotropy much attention is given to a full discussion of the 'topical parameters,' first proposed by Muthmann and Becke, by means of which it is possible to compare to better advantage than was hitherto possible the crystal structure of different substances as well as to note the variation caused in their crystallization by changes in chemical composition.

The English translation by Hugh Marshall, of the University of Edinburgh, is in every respect admirable. A copy of this introduction ought to be in the hands of every chemist.

The 'Chemische Krystallographie' proper is to comprise four volumes as follows: Volume I., Elements, inorganic binary compounds, simple and complex haloids, cyanides, nitrites and their alkyl compounds of the metals; Volume II., inorganic oxy- and sulfo-salts and their alkyl compounds; Volumes III. and IV. will contain the organic compounds. Of these only the first volume has been published. The remaining volumes are to appear at intervals of one year.

The arrangement of Volume I., which is also to be followed in the others, is such that substances, which are similar chemically are treated together in separate groups or sections by first discussing our present knowledge of the same. These discussions present a very clear and concise survey of the literature, point out the conclusions to be drawn concerning the crystal structure of the substances under consideration, and in many instances indicate important lines of needed research. The second portion of each section is devoted to a systematic description of the members of that group for which crystallographic data

were obtainable. These data are uniformly complete and usually accompanied by numerous figures. For example, 42 pages and 40 figures are devoted to the elements. In this chapter on elements there are no less than 237 references to literature given. The monohaloids are described in 40 pages containing 50 figures and 147 references, of which over two pages and six figures are given to ammonium chloride alone.

Chemists, crystallographers and mineralogists have long felt the need of a good reference work of this character and are greatly indebted to Professor von Groth for placing at their command in a clear and concise form such a vast amount of information concerning crystallized bodies. The appearance of the remaining three volumes will be awaited with much interest.

Edward H. Kraus
Mineralogical Laboratory,
University of Michigan,
December 14, 1906

Principles of Botany. By Joseph Y. Bergen, A.M., and Bradley M. Davis, Ph.D. Boston, Ginn & Company. 12mo. Pp. x + 555.

Ten years ago Mr. Bergen, then instructor in biology in the English High School of Boston, brought out an admirable little book entitled 'The Elements of Botany' designed to be a text-book for use in the high schools. It soon became deservedly popular and was very widely used. Five years later there appeared 'The Foundations of Botany,' a much larger book, in which the author, after revising the chapters of his earlier book, had injected a good deal of the new branch of botany—ecology—accompanied with a considerable number of half-tone and other illustrations of leaf-patterns and landscapes, in accordance with the ecological fashion of that day. In the book before us, we have a further modification of the author's idea of the kind of matter to be presented to the young beginner in botany in the high school, and perhaps the first course in college. In its preparation the author associated with him Dr. Davis, until recently of the University of Chicago, so that it appears under their joint authorship.

After a brief introduction, mainly devoted to a definition of botany and its subdivisions (morphology, physiology, plant geography, paleobotany, taxonomy, ecology and economic botany), we have the remainder of the book divided into three parts, viz., I., 'The Structure and Physiology of Seed Plants' (146 pages), II., 'The Morphology, Evolution and Classification of Plants' (257 pages), and III., 'Ecology and Economic Botany' (129 pages). Parts I. and III. are the work of the senior author, while Part II. is from the hand of Dr. Davis.

Part I. is a still further revision of the first dozen or so chapters of the 'Foundations.' The treatment is much briefer, and all 'experiments' are left out, so that instead of 227 pages in the 'Foundations' only 146 pages are given to this portion of the subject in the 'Principles.' Part II. is entirely new matter, and is an admirable presentation of the elements of systematic botany. Dr. Davis has shown his ability to present an outline of this vast subject in such manner as to give the student a clear picture of the whole. The only criticism of this part of the book is that it will probably be found to be quite too full, and perhaps too difficult for pupils in secondary schools, and better adapted to the capacity of college students. Part III. is based upon the second part ('Ecology') of the 'Foundations,' containing, however, much new ecological matter, which is well and clearly presented, and several chapters on economic botany which do not appear to be necessary in a book of this kind. One may seriously question the usefulness to beginning students of chapters including such topics as plant breeding, the production of hybrids, selection among corn, selection among wheat, results of hybridizing citrous fruits, and wheat, food products for human use, and for domestic animals, plant-fibers, timber, forestry and fuel. These subjects can not be adequately treated in an elementary text-book intended for children. The little that is said under each topic is not enough to serve as a beginning of the subject, and there is certainly neither space nor time for more. It has often

been said that the most difficult task in the preparation of an elementary text-book is to make a judicious selection of the things to be included from the vast multitude of things which present themselves. To know what he may safely exclude, and yet make a connected story, which shall be brief enough to be mastered in the time at the student's disposal, is, we admit, not easy to accomplish. To 'touch the high points' and yet to keep up the connection between them is the difficult task of the writer of an elementary text-book. In some portions of the book before us this has been accomplished, while in others a good deal of matter has been admitted which might well have been left out.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Comparative Neurology and Psychology for January includes a paper 'On the Place of Origin and Method of Distribution of Taste Buds in *Ameiurus melas*,' by F. L. Landacre, a study of the embryology of the taste buds of the catfish. He shows that taste buds appear simultaneously in the entoderm of the gill arches and in the ectoderm of the lips. From both of these centers the buds spread backward, from the first into pharynx and œsophagus and from the second into the mucous membrane of the mouth and also into the outer skin, finally reaching the extreme dimensions of the outer surface of the body. No buds migrate from entoderm to the skin. The series of papers on the nervous mechanisms of touch and taste in fishes by C. J. Herrick is continued by 'A Study of the Vagal Lobes and Funicular Nuclei of the Brain of the Codfish.' Instructive comparisons are drawn between the central mechanism of this fish and Ameiurus and an attempt made to explain their difference on the basis of the mode of life of the fishes. There is also given a translation of the recent researches by Minkiewicz on 'Chromotropism and Phototropism.'

SOCIETIES AND ACADEMIES

THE AMERICAN PHILOSOPHICAL SOCIETY.

A STATED meeting was held on January 4, at 8 o'clock. Professor J. C. Branner communicated a paper on 'The Geology of the San Francisco Peninsula,' by Roderic Crandall.

DISCUSSION AND CORRESPONDENCE

THE 'FIRST SPECIES RULE' VS. THE 'LAW OF PRIORITY' IN DETERMINING TYPES OF GENERA

IN connection with the discussion on 'elimination' vs. 'first species,' in determining type species, may I be permitted to bring forward certain points which seem to me to be worthy of consideration?

That some authors are decidedly opposed to 'elimination,' while others are equally opposed to 'first species,' indicates rather strongly that there are valid objections to both methods, or at least that neither method is perfect. Whatever our views in the case may be, it is a matter of record that some authors have adopted the one method, while other authors have adopted the other.

If a given rule of nomenclature is to command the general respect of biologists and not to be subject to change from generation to generation, it should be sufficiently just, objectively, to appeal to all persons who are called upon to apply it and who may be temporarily inconvenienced by its application. The question, therefore, arises whether the 'first species' rule is so inherently just in principle that it will appeal to systematists to sufficiently convince them of the justice of overturning hundreds or possibly thousands of cases of type determination which have been made since 1758, and especially since 1842.

Personally, I view the first species rule as one of enormous convenience, and as one which can be applied, in the vast majority of cases, uniformly by all workers.

That it is necessary (however desirable it may be) to have a rule which will apply uniformly to all genera, is a point which I very seriously doubt. On the contrary, it seems to me that there is a certain amount of advantage in allowing a margin for the exercise of some discretion in certain cases. That two authors may arrive at different conclusions on the basis of elimination does not, therefore, seem to me to condemn it.

My friend Dr. Allen has given *Vultur* Linnæus, 1758, as an example of elimination. All authors may not agree with him in his method of elimination, but I believe I am justified in pointing out that both he and those authors who differ with him as to his method of elimination (for two methods are possible in this case) have overlooked the very important fact that they have ignored the rule which Linnæus himself laid down for the determination of types of his own genera. Now, since Linnæus did give a rule to be applied to his genera, it seems to me that it is obligatory to apply that rule to Linnæan genera, regardless of our views of first species or of elimination. The example upon which the discussion is based is, therefore, an invalid one from either Dr. Allen's or Mr. Stone's point of view.

A point to which attention may be directed is that, not only the elimination rule, but the first species rule also is interpreted differently by different men. Take the genus *Dispharagus* Dujardin, 1845, for instance. If an author who does not adopt the rule of type by inclusion were to determine a type for this genus, he might select either *D. decorus* or *D. laticeps* or *D. tenuis* as type, and yet he would be following the first species rule, as interpreted by three different sets of workers. It should, however, be mentioned that if he followed the rule as interpreted by the American Ornithological Union, this difference of opinion would not arise; but not all first-species men agree with the American Ornithological Union interpretation.

Still more important than the foregoing, is the fact that the first species rule would in many cases virtually misrepresent an original author's intention, in that if the type is determined on basis of his generic diagnosis and with a number of species from which to select, the nature of his genus becomes more or less clear and we have a more or less definite systematic unit upon basis of which we can confidently proceed with further work. If, however, the first species rule is made ironclad, then not infrequently will the genus be based upon a very imperfectly described species and will thus be more or less obscure, hence further systematic work based upon such determination unnecessarily runs the risk of being only ephemeral in character. We would thus make ourselves slaves to a rule of convenience (the convenience being judged from only one standpoint), rather than make the rules our servants.

Again, in many groups there is an exceedingly great advantage in taking a figured species as type. In view of the possible necessity of a future restudy of the anatomy of the type species, in order to determine some point which was not foreseen, it is often of great importance to select as type a species which is common and, therefore, easily obtained. Nor should we forget DeCandolle's excellent advice as to the value of selecting as type a species belonging to a group which contains as large a number of species as possible, in order to change as few names as possible. These are other points of view in reference to 'convenience,' points of view which are important not alone to the systematist, but also to the morphologist, while the first species rule ignores the morphologists entirely.

In return for ignoring these points, the first species rule presents to us one one-sided advantage. It is a rule of convenience—not of principle, and as such it relieves an author from knowing the literature when he attempts systematic work. My very esteemed colleague, Mr. Stone, expresses it, at least inferentially, in another way, namely, he compares the first species rule to arithmetic, hence he argues that any one can use it, while the more complicated methods he compares to higher mathematics. The comparison is an excellent one, and I accept it. But it may be remarked that the question of type determination is one which involves so many points that it should not be entered into by any person whose education has not extended beyond the arithmetic stage; like astronomy, it calls for higher mathematics; and to my mind the application of the first species rule to type determination will be found, when extended to all zoological groups, to be about as satisfactory to the systematists at large, as would arithmetic be as a final mathematical process in the case of astronomy.

I can not, therefore, see in the first species rule any inherently just principle, nor can I see in the processes which it is designed to supplant any corresponding inherently unjust principle, which indicates that future generations of zoologists would abide by the rule if adopted. Accordingly, I am unable to view this proposed legislation as advisable.

All, or practically all, systematic zoologists recognize that the principle of priority is inherently just. It commands respect, even though it irritates us occasionally. We apply it to generic names, without a murmur, or at least without murmuring very loud. If this principle is just when applied to the generic names, why is it not equally just when applied to the generic types? In the one case as in the other, the author who applies it must know the literature. As a matter of fact, the status of no generic name is satisfactorily established, from the modern point of view, until the type is designated. But when this type is once designated, by any method whatsoever, so long as the species selected was an original species, valid from the original author's point of view, and unreservedly classified in his genus, why reopen the question? At that date the generic name first complied with all of the formal conditions which can reasonably be demanded of it. Why now reverse the decision of the author who took this step, even if you or I would have done it in a somewhat different manner? If he selected the type on the first species rule, or if he did so on some other rule, or on no rule at all, the point can still be objectively demonstrated that the type was actually designated. This point being established, the question should be settled once for all. A genus can not have two separate type species; if, therefore, any author has definitely designated a type species for any given genus (regardless of his method), how can we establish another type species for it? To do so, by legislation or otherwise, is to weaken the very foundation of nomenclature —namely, the principle of the law of priority.

The discussion on this very live subject in nomenclature has convinced me more than ever of the justice of a rule to the effect that no new generic name published after a given date, say January 1, 1908, shall be entitled to consideration unless its author definitely designates a type at the time of its publication. If American zoologists approve of this proposition (several systematists have already signified their approval), I am willing to do what I can to have it inserted in the International Code. I believe it would be wiser to make such a rule retroactive (namely, to date all genera from the time their types were designated) than to adopt the first species rule at this late day.

CH. WARDELL STILES

WASHINGTON, D. C.

THE FIRST SPECIES RULE VERSUS ELIMINATION

DISCUSSIONS concerning the adoption of the first species rule for fixing the types of genera have been so generally accompanied by extravagant statements of the probable revolution that would be thus occasioned in our nomenclature that there seemed to be a need for some statement of the matter based on fact and not on theory, and my recent article in SCIENCE was intended largely to supply this need. I had no thought of starting a lengthy controversy, nor do I desire to do so now. As my friend Dr. Allen in his recent comments upon my paper relies mainly upon general statements and does not prove any of my facts or figures to be inaccurate, he does not impair the strength of my argument and there would be no call for a reply were it not that he claims that I have been (doubtless unconsciously) led into a few misleading statements. These so far as I gather from his article are:

1. "That elimination has never been practised in Europe and does not seem to be understood by foreign writers." I was perfectly well aware that the 'first reviser' principle was incorporated in the B A Code of 1842, and in most others, *i. e.*, "that when no type is indicated the author who first subdivides a composite genus may restrict the original name to such part of it as he may deem advisable." But I claim that so far as birds are concerned the first revisers in the vast majority of cases have restricted the original name to the first species and its allies and

that when they failed to do so, subsequent authors have frequently ignored them and have selected the first species as the type. Furthermore, European authors have not practised the kind of elimination that shifts *Passerina* on to the snowflake and *Sarcorhamphus* on to the eared vulture, and this sort of name shifting is what I claimed to be not understood[1] abroad.

Moreover, when we find that out of 277[2] complex genera of birds the currently accepted types of only 38 would be changed by the operation of the first species rule I am forced to believe that the first species was very generally regarded as the type by the first revisers and that the result is not a mere 'coincidence.'

2. Dr. Allen states that the rules and recommendations of Dr. Stiles referred to by me 'relate only in small part to the method of elimination' and cover the whole field of the determination of generic types, including the 'four conditions' of (1) monotypic genera, (2) type designation by the author, (3) tautonomy and (4) selection of type by subsequent author.

This is perfectly true as applied to Dr. Stiles's rules as a whole, but he has twenty-four rules and recommendations and Dr. Allen will find that I referred to only nineteen, omitting those covering the first three conditions stated above. It is true that I did include the 'first reviser prerogative' which Dr. Allen in this connection implies is not elimination. It seems to me, however, to be so intimately associated with the operation of elimination as to be inseparable from it, and Dr. Allen himself says on p. 773 that with the adoption of the 'first reviser' rule 'the elimination principle follows as a necessary corollary.' The thirteen secondary suggestions to which I referred all relate to elimination in its strictest sense.

3. On p. 775 Dr. Allen makes a statement that I fail to understand, *i. e.*, "that the first species method is 'not always so simple and direct' as I have stated and that the case of *Vultur* will show that more than one reference must be consulted even under the first species rule." I have searched in vain for any demonstration of this claim in the subsequent pages of Dr. Allen's paper. Surely to ascertain the first species mentioned by an author in describing a new genus we have only to look at his original description! Dr. Allen must certainly have misunderstood the first species method here and also at the bottom of p. 776, where he says it would conflict with the 'rule that a monotypic genus takes its sole species as its type.' If *barbatus* had been the first species in *Vultur*, as he suggests, it would of course be the type, but this would in no way affect the type of the monotypic genus *Gypaëtus* which would remain *barbatus*. *Gypaëtus* being of later date would of course be a synonym of *Vultur* just as it would have been if *barbatus* had been the only species in *Vultur* or if it had been designated by Linnæus as the type of *Vultur*. This argument simply shows that genera with the same types are synonyms and has no further bearing.

4. Dr. Allen at p. 778 calls attention to the fact that "by the first species rule, where the first species happens to be the same in two or more genera * * * all the later genera become pure synonyms of the earliest genus" and then goes on to say: "It is thus evident that Mr. Stone's statistics greatly underestimate the number of changes in names that would result from the adoption of the first species rule." This deduction is entirely unwarranted. It assumes that I overlooked the synonymizing of genera with the same first species. This I did not do and all changes due to this cause are included in my statistics.

[1] I regret that this word has proven misleading. I had no intention whatever to question the ability of our friends across the water to practise elimination as Dr. Bather supposed, but simply that they did not *interpret* the method in the way Americans have done.

[2] Since my paper was published I have continued my card list of bird genera to 1830. Up to that date I have 1,119 genera, of which 842 are either (1) monotypic, (2) have their types designated by their authors, (3) indicated by tautonomy or (4) are substitutes, leaving 277 with no indication of type, and in 86 per cent. of these the first species is the currently accepted type according to the British Museum catalogue.

As an argument against the first species rule this has no weight, as it applies with equal force to any method of fixing types. I might say, for instance, "if the types of two or more genera happen to be the same by elimination the later genera become pure synonyms of the earliest." *Otogyps* is suppressed as a synonym of *Sarcorhamphus* by this very method in Dr. Allen's paper.

So much for my 'misleading statements.' Turning now to Dr. Allen's elaborate discussion of the types of the Vulturine genera, which he gives as an example of how elimination should be practised and which we should be very glad to see, as it gives us an actual case or series of cases worked out by one who is a recognized expert in this method of fixing types.

My chief objection to the method (*i. e.*, elimination) is that it will give different results in the hands of different workers owing to the almost infinite variety of ways in which it may be applied. Dr. Allen, far from refuting this claim, actually shows that two different methods of elimination may (no doubt unconsciouly) be used by the same author in the same paper, thus emphasizing the elasticity of the method and the impossibility of formulating rules that will meet all its varied requirements.

Any one who has practised elimination knows that there are two methods in use in successively removing the species of a genus which have been made the basis of subsequent genera.

(*a*) Some remove only the species which has been made the type of a subsequent genus at the date at which the genus was established.

(*b*) Others remove along with the type any other strictly congeneric species, and here again there are two practises according as we interpret congeneric to mean congeneric from the standpoint of the author of the genus, or congeneric from the standpoint of the eliminator.

Taking Dr. Allen's elimination of *Sarcorhamphus* at the top of p. 776, he says:

> *Sarcorhamphus*, 1806; species *gryphus, papa, auricularis*. The species *papa* was removed to *Cathartes* in 1811, *gryphus* to *Gypagus* in 1816, leaving *auricularis* as the type of *Sarcorhamphus*.

The species thus removed are not, according to Dr. Allen's conclusions, the types of the genera *Cathartes* and *Gypagus*, but they were included in these genera by their authors in 1811 and 1816, respectively. It will thus be seen that Dr. Allen adopts method '*b*' (above) in his elimination and interprets 'congeneric' to mean congeneric from the standpoint of the original author, not from that of the eliminator (or the usage of the present day). Having fixed the types of the four involved genera in this way, he next proceeds to eliminate *Vultur* by removing the species *at the dates at which they became the types* of subsequent genera—*i. e.*, according to method '*a*.'

If *Vultur* were eliminated in the same way as *Sarcorhamphus* the result would be as follows:

> *Vultur*, 1758; species *gryphus, harpyja, papa, aura, barbatus, percnopterus*. The species *barbatus* was removed to *Gypaëtus* in 1784, *gryphus* and *papa* to *Sarcorhamphus* in 1806, *percnopterus* to *Neophron* in 1808, *aura* to *Cathartes* in 1811, leaving *harpyja* as the type of Vultur.

If we do not trouble ourselves to ascertain the types of *Cathartes* and *Gypagus* when we eliminate *Sarcorhamphus*, I fail to see why we have to ascertain the types of the involved genera when we eliminate *Vultur*.

As a further example of the various ways in which elimination may be practised, it will be noticed that Dr. Allen pays no attention to what may have been done to species prior to the date of the genus that he is eliminating. Under *Gypagus*, 1816, he says: "*gryphus* was removed to the genus *Gryphus* in 1854," but as a matter of fact it had already figured in the establishment of the genus *Sarcorhamphus*, 1806, and proves, according to Dr. Allen's demonstration, to be the type of *Vultur*, 1758. Here again very different results may be obtained according as we consider or ignore the work of authors prior to the date of the genus we are eliminating.

Dr. Allen truly says that elimination requires 'a thorough knowledge of the literature of the cases involved' and 'is therefore

not a task a novice should meddle with.' This is another great objection to the method, since we never know when we have exhausted the literature and so never know when we have our types definitely fixed, while the worker who has not an enormous library at his command is unable to attempt to settle the application of his genera.

In the *Vultur* case, Dr. Allen, whose knowledge of ornithological literature is equaled by few, has overlooked two genera, *Rhinogryphus,* 1874, and *Torgos,* 1828, which, respectively, antedate *Œnops* and *Otogyps.* Fortunately for his eliminations these are both monotypic and their dates are such that they do not alter the results. If they had been proposed some years earlier, however, they would not only have replaced the above genera, which they do in any case, but by removing their species from other genera at earlier dates they would have altered the results of several of Dr. Allen's eliminations.

If *Torgos,* for instance, had been 1815 it would have left *gryphus* as the type of *Sarcorhamphus* instead of *auricularis,* while *Rhinogyphus* at 1815 would have left *papa* as the type of *Cathartes* instead of *aura,* and by Dr. Allen's method the type of *Vultur* would then have been *harpyja.* In other words, the discovery of two overlooked genera would not only replace two current genera by reason of priority, but would *by elimination* alter the types of three other genera. With the types fixed by the first species rule the only effect of the resurrection of the old names would be their substitution for the two current names having the same types.[3]

The *Vultur* text invites one more comment. Dr. Allen states that by ignoring 'the fixing of a type by a later author' I have 'needlessly increased the number of open cases by from probably 50 to 75 per cent.' Now as a matter of fact the fixing of a type by a later author has no status whatever in the eyes of those who practise elimination *unless it agrees with the action of revisers* up to the time that the type was so fixed. Therefore the cases are more open under the operation of elimination than if we settled them once for all by taking the first species of the original publication as the type. For example, the types of *Cathartes, Sarcorhamphus* and *Gypagus,* the three genera most involved in this Vulturine muddle, were definitely fixed by Mr. Ridgway in 1874, and independently by Dr. Bowdler Sharpe in the same year, each selecting the same species, as follows:

Sarcorhamphus, type *gryphus.*
Cathartes, type *papa.*
Gypagus, type *papa.*[4]

We might infer from Dr. Allen's statements that this settled the cases of these genera for all time, for he says: "There are four conditions, any one of which when present determines the type of a genus *beyond appeal* [italics mine] under current usage" and as the fourth condition he gives "4. When some subsequent author has selected one of its [*i. e.,* the original genus] species as its type."

Nevertheless, he ignores absolutely the action of these two eminent type-fixers and opens all these genera to elimination with the following results:

Sarcorhamphus, type *auricularis.*
Cathartes, type *aura.*
Gypagus, type *papa.*

It seems, therefore, that the action of a later author in fixing the type of a genus is *not* 'beyond appeal' and 'condition 4' needs an important amendment. Further examples of the unsatisfactory nature of elimination might be drawn from this case of *Vultur,* but I fear I shall be charged with rivaling the combined vision of Romulus and Remus on

[3] In spite of what Dr. Allen says on p. 777, the first species rule will give the same relief in cases where the type of one genus depends on whether or not two other groups are regarded as congeneric or not. *Cf.* Jordan, SCIENCE, 1901, Vol. XIII., p. 500, where the first species rule as advocated in my paper is formally proposed.

[4] It is interesting to note that both Mr. Ridgway and Dr. Sharp have in each instance selected the *first species* as the type and one would be inclined to suspect that they were following, consciously or unconsciously, the first species rule, though it may have been merely a 'coincidence' as Dr. Allen suggests in another connection.

the hills of ancient Rome in the number and variety of Vultures that I have been able to discern.

With Dr. Allen's closing statement that the first species rule 'has only here and there a disciple' or that it has ever been generally abandoned *in practise* so far as ornithology is concerned, I beg to differ.

The interviews and correspondence that I have had since my paper was published show that the adoption of the first species rule as there outlined meets with very general approval among vertebrate zoologists as well as entomologists, while botanists, as is well known, have long practised it.

One prominent entomologist in a recent publication hopes that it may be incorporated in the International Code at an early date, while one of the foremost zoologists of America writes me that "elimination is absolutely dead and ought not to be revived in any code or thought of in any connection."

A thorough discussion of this subject is desirable, but really, my friend Dr. Allen and I are of nearly the same mind on the question. He says at the beginning of his article: "I have always conceded that this [*i. e.*, the first species principle] would be the ideal method if we were at the threshold of our work * * * and my opposition to it has been * * * that to adopt it now would introduce serious confusion into nomenclature." This was exactly my view, and when upon investigation I found that serious confusion (so far as birds are concerned) would not ensue, I thought that there were no further grounds for objection. The other objections that have occurred to Dr. Allen in the later pages of his paper I have tried to dispel.

At the present time I feel more sure than ever that the zoological code that adopts the first species rule (excepting in relation to Linnæus) will be setting an example which will in a few years be followed by vertebrate zoologists in general and, with a possible further limitation, by invertebrate zoologists as well.

WITMER STONE
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SPECIAL ARTICLES

ON A CASE OF REVERSION INDUCED BY CROSS-BREEDING AND ITS FIXATION[1]

PERHAPS the most important extension which has been made of the law of heredity originally discovered by Gregor Mendel consists in the demonstration (chiefly by Cuènot and Bateson) that certain characters are produced only when two or more separately heritable *factors* are present together. Such a character does not conform with the simple Mendelian laws of inheritance, but its *factors* do. Herein lies the key to the explanation of so-called *heterozygous* characters and to the practical process of their fixation. This same principle serves to explain also atavism or *reversion,* and the process by which reversionary characters may be fixed.

When pure-bred black guinea-pigs are mated with red ones, only black offspring are, as a rule, obtained. The hairs of the offspring do indeed contain some red pigment, but the black pigment is so much darker that it largely obscures the red. In other words, black behaves as an ordinary Mendelian dominant. In the next generation black and red segregate in ordinary Mendelian fashion, and the young produced are in the usual proportions, three black to one red. All black races behave alike in crosses with the same red individual, but among the reds individual differences exist. Some, instead of behaving like Mendelian recessives, produce in crosses with a black race a third apparently new condition, but in reality a very old one, the agouti type of coat found in all wild guinea-pigs, as well as in wild rats, mice, squirrels and other rodents. In this type of coat red pigment alone is found in a conspicuous band near the tip of each hair, while the rest of the hair bears black pigment. The result is a brownish or grayish ticked or grizzled coat, doubtless inconspicuous and so protective in many natural situations. Some red individuals produce the reversion in half of their young by black mates, some in all, and others, as we have seen, in none, this last condition being the commonest of the three. It is evident that the

[1] Published by permission of the Carnegie Institution of Washington.

reversion is due to the introduction of a third factor, additional to simple red and simple black. It is evident further that this new third factor, which we will call *A* (agouti), has been introduced through the red parent, and that as regards this factor, *A*, some individuals are homozygous (*AA*) in character, others are heterozygous (transmit it in half their gametes only), while others lack it altogether. Further observations show that it is *independent* in its inheritance of both black and red; it is, in fact, an independent Mendelian character, which can *become visible* only in the presence of both black and red, because it is a mosaic of those two pigments.

The reversionary individuals (agoutis) do not breed true. They produce offspring of three sorts, agouti, black and red. When mated *inter se*, they produce young in the proportions, 9 agouti: 3 black: 4 red; when mated with reds (recessives), they produce young in the proportions 1 agouti: 1 black: 2 red.

It has been found by experiment that the F_2 agoutis (produced by mating the original agoutis *inter se*) are of three different sorts. It is probable that they are in reality of four different sorts. The three sorts thus far recognized are (1) agoutis like their parents, which produced the three sorts of young, agouti, black, and red in the proportions already stated; (2) agoutis which in matings with recessives (reds) produced only agouti young and red young in equal proportions; (3) agoutis which in matings with red animals produced *no red offspring*, but only agouti ones and black ones in equal numbers. The fourth (expected) class should when mated with reds (or any other color, for that matter) produce only agouti colored young; *i. e.*, should not only *resemble* wild guinea-pigs in the character of its coat, but should transmit this character to all its young, as wild ones do. This is on the hypothesis already stated that the factor *A* is a distinct Mendelian character. The gametic constitution of the four classes of agoutis would on this hypothesis be:

1. *RA·B*, forming gametes *R*, *RA*, *B*, and *BA* with equal frequency.

2. *RA·BA*, forming gametes *RA* and *BA* with equal frequency.

3. *BA·B*, forming gametes *BA* and *B* with equal frequency; all these classes have been proved to exist.

4. The fourth (expected) class, the fully fixed agouti, known to exist but not yet produced in this experiment, should have the formula *BA·BA* like class 2 in that *A* is transmitted in all its gametes, but differing from it in that black also is transmitted in *all* the gametes, instead of in half of them only.

But, some one may inquire, how is it that an individual which forms *no* red gametes (as *B·BA* or *BA·BA*) can nevertheless produce agouti young, which character is by hypothesis a mosaic of red with black. This is a matter which gave me considerable trouble and made the at first wholly unexpected class 3 (*B·BA*) for a long time perplexing. The explanation is this: Ordinary black individuals, while homozygous, are not pure in the sense that they form no pigment but black. They probably always form a certain amount of red and of brown pigment, which is overlooked only because it is interspersed with the more opaque black pigment. If the red pigment is segregated, as is the case when the factor *A* is present, then it becomes visible as the agouti marking of the hair. If this factor, *A*, is present in both halves of the zygote together with black (and enough red to form the agouti hair tip) a homozygous agouti animal is the result (class 4); if *B* is present in both halves of the zygote, but *A* in one half only, agouti and black young will be produced.

The existence of a third factor, *A*, in cases of reversion in coat-character among rodents has been heretofore overlooked merely because it does not represent a distinct pigment or set of pigments, but consists in a particular kind of pigment distribution on the individual hairs. The agouti hair is due to a definite cycle of activity of the hair follicle in forming its pigments, first black, then red, then black; the wholly black hair is due to a continuous process of pigment formation without alternation in the character of the pig-

ments produced. The relation between agouti and black is precisely the same as that between short hair and long hair due likewise to differences in follicle activity, as I have elsewhere shown, but inherited quite independently of hair pigmentation. Short hair is the result of a determinate growth cycle; the hair grows so long and then stops growing; long or angora hair is the result of indeterminate activity on the part of the hair follicle; the hair keeps growing so long as its follicle is alive.

We are now able to give a rational explanation of the *origin* of the various color varieties of rodents. The wild cavy transmits *in all its gametes* the three factors *A*, *B* and *R*. By accident (mutation) a gamete has been formed which lacked *A*. When two such gametes came together the result is a black individual, *and this individual will breed true*. Here is the explanation of our occasional black squirrels, porcupines and the like. If by a further mutation *B* is lost, leaving *R* alone, a red race is produced which will breed true and *will not give reversion on crossing with blacks*. Such are ordinary red guinea-pigs.

But if mutation is directly from the wild or agouti condition, *ABR*, by loss of *B*, leaving *AR*, then there is produced a red not different from ordinary reds in appearance, but which will give reversion in crosses with black.

The albino mutation, which is frequently found in wild as well as in tame rodents, is not due, as might be supposed, to simultaneous loss of the three factors *A*, *B* and *R*, for albinos can be shown to possess, some one, some two and some all three of these factors. They have, according to Cuènot, lost a certain other factor necessary for the production of pigment of any kind, an activating or ferment-like factor.

It has been observed that one mutation is often followed by another. De Vries in his *Mutationstheorie* speaks repeatedly of *periods of mutation*. We can begin to see the significance of this; given one mutation, we can produce others.

Suppose, for example, that we possess agouti and ordinary red varieties only and desire black, we are not compelled to await a mutation to produce it; we can cross red with agouti and obtain black in the second generation. This is not hypothesis merely; its correctness has already been in part demonstrated. Thus, in one experiment, there was employed an agouti of the formula $AB \cdot AR$, which gave only reds and agoutis in crosses with red, but the agoutis so produced when mated in the same way as the parent gave blacks as well as reds and agoutis, for they were of the formula $AB \cdot R$. From such animals homozygous blacks ($B \cdot B$) are readily obtained.

To produce a red variety from agoutis and blacks alone would not be so easy; it would be necessary either to await a mutation or to work by the slow process of selection from continuous variations in the intensity of blacks under cross-breeding with agoutis. In mice and rabbits as well as in guinea-pigs red (or yellow) varieties are well known, but in rats yellow has never been obtained separate from black, though black and agouti varieties are common, both wild and in captivity.

We now know what the 'fixation' of a heterozygous character implies. When *A* and *B* are crossed, we obtain *C*. *C* is due either simply to co-existence of *A* with *B*, or to the co-existence with them of a third factor introduced with one or the other. In either case fixation will consist in getting *into the gamete* all the factors which produce *C*. In the first case, the zygote is $A \cdot B$, and the resultant is equivalent to *C*. Fixation will consist in getting a zygote of the formula $AB \cdot AB$; every gamete produced will then bear the equivalent of *C*, viz., *AB*. In the second case, the zygote is either $AC \cdot B$ or $A \cdot CB$; fixation will consist in obtaining a zygote, $ACB \cdot ACB$; every gamete formed will then contain the three factors, *A*, *C* and *B*.

W. E. CASTLE
ZOOLOGICAL LABORATORY,
HARVARD UNIVERSITY,
December 26, 1906

BOTANICAL NOTES

THE RUSTS OF AUSTRALIA

UNDER this title D. McAlpine, the government pathologist of Victoria, Australia, prepared a book of 350 pages, which has been

issued by the Department of Agriculture of that province. In it the author "aims at recording all rusts, as far as known in Australia at present," so as "to prepare the way for a consideration of the best methods of preventing their appearance, or limiting their spread in the numerous commercial crops subject to their ravages." Part I., consisting of 75 pages, is devoted to the general characters and mode of life of the rusts (*Uredineae*). This portion would be a very helpful text-book for college students anywhere, since the matter is presented in a clear and comprehensive manner. It appears that in Australia *Puccinia graminis* does not infect the barberry, even where the attempt is made to bring about such infection by artificial means. This is much like the condition which prevails on our own great plains, where wheat rust is sometimes very abundant, although there may be no barberry plants in the neighborhood, or if these are present they may have no aecidiospores upon their leaves. Part II., which includes 260 pages, is devoted to classification and technical descriptions. The arrangement here is principally by hosts, the rusts of the *Gramineae* being taken up first, and then those of *Cyperaceae, Juncaceae, Liliaceae, Haemodoraceae, Amaryllidaceae,* etc. Fifty-five plates (eleven of them beautifully and accurately colored) help to render the descriptions more easily understood. A glossary of technical terms, a bibliography, an alphabetical host index, a fungus index (alphabetical by genera) and a general index complete this very satisfactory volume.

POPULAR CANADIAN BOTANY

This appears to be the day of popular botany of a type somewhat different from that which used to be prepared for the perusal of the non-scientific reader. All the books on the ecological phase of botany owe much of their readableness to the fact that they are popular in a certain sense. They tell a good deal about plants and vegetation in language that may be understood by people who are not experts in systematic botany. The scientific lists of plants which used to be published were very illuminating to those botanists who knew plants by their latest Latin names, but they conveyed only the haziest ideas to other people, even though they were botanists, in some other field. So when we found books in which there were consecutive pages of 'reading matter' telling something about the kinds of vegetation in a field, a county or a state, no wonder that a good many of us rejoiced that at last we had the means of finding out about the plants of a region without the labor of building up a picture of its vegetation from the bare lists. All this time also there were popular books which aimed to please the non-scientific reader by presenting the beauties, the oddities, the curiosities in nature. Such books usually dwelt upon the wonders which the reader might see if only he was willing to open his eyes in the right way—the way of the author, of course. That such books are not yet extinct is shown by the republication, after revision, of Mrs. Traill's 'Studies of Plant Life in Canada' (W. Briggs, Toronto), a book of somewhat more than two hundred pages of descriptive text, accompanied by twenty plates, a part of them done in colors. The text is likely to appeal to many young people who have not yet waked to an appreciation of the scientific aspects of botany, and it will appeal, also, to many older persons who look at all vegetation as something to be admired and enjoyed without too much hard study. It has been recommended by several of the Canadian botanists as a book for use in nature study work in the public schools, and no doubt it might be helpful in such case if used as a reading text for the purpose of suggesting the notice of the many pretty and attractive plants to be found everywhere. Since poetry and Scripture are freely quoted the moral effect of the book is likely to be elevating, at least the author has distinctly intended it to be so.

THE PHILIPPINE JOURNAL OF SCIENCE

The announcement is made that beginning with the second year of its publication *The Philippine Journal of Science* will be issued in three divisions or series, viz., (*a*) general science, (*b*) medical science, (*c*) botany. This

will enable botanists to subscribe for the botanical parts alone, a considerable convenience, since the subscription price is only two dollars (U. S. currency) for the botanical series, instead of five dollars, as heretofore for the whole journal. As this publication is of constantly increasing importance to American botanists, it is hoped that it will be generously supported. Subscriptions are to be sent to the Director of Printing, Manila, P. I.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

FELLOWS ELECTED AT THE NEW YORK MEETING OF THE AMERICAN ASSOCIATION

SECTION A, MATHEMATICS AND ASTRONOMY

Baker, Robert Horace, Amherst, Mass.

Bliss, Gilbert Ames, Princeton, N. J.

Bowie, William, Coast and Geodetic Survey, Washington, D. C.

Brown, George Lincoln, Brookings, S. D.

Dugan, Raymond S., Princeton, N. J.

Faught, John Brookie, 1312 Presque Isle Ave., Marquette, Mich.

Glenn, Oliver Edmunds, 1227 Clay Avenue, Springfield, Mo.

Graham, William Joseph, 1 Madison Avenue, New York, N. Y.

Granville, William Anthony, Yale University, New Haven, Conn.

Hadley, Stephen M., Oskaloosa, Iowa.

Leavitt, Henrietta Levan, Harvard College, Cambridge, Mass.

Lowell, Percival, 53 State Street, Boston, Mass.

Maclay, James, Columbia University, New York, N. Y.

Manning, Henry Parker, Brown University, Providence, R. I.

Olds, George Daniel, Amherst, Mass.

Plimpton, George Arthur, 70 Fifth Avenue, New York, N. Y.

Poor, Charles Lane, 4 East 48th Street, New York, N. Y.

Riggs, Norman C., Armour Institute, Chicago, Ill.

Smith, Franklin Hans, P. O. Box 762, Denton, Texas.

Snyder, Z. X., Greeley, Colo.

Washburne, Alva Courting, c/o Hartford Mutual Life, Hartford, Conn.

Wilson, Norman Richard, Winnipeg, Canada.

Young, Anna Sewell, South Hadley, Mass.

SECTION B, PHYSICS

Blaker, Ernest, 402 Oak Avenue, Ithaca, N. Y.

Burgess, Geo. K., Bureau of Standards, Washington, D. C.

Chamberlain, C. W., Denison University, Granville, Ohio.

Earhart, R. F., Ohio State University, Columbus, Ohio.

Fox, Wm., College of the City of New York, New York, N. Y.

Gates, Fannie Cook, Woman's College, Baltimore, Md.

Hower, Harry S., Carnegie Technical Schools, Pittsburg, Pa.

Lyman, Theodore, Harvard University, Cambridge, Mass.

Ramsey, Rolla R., Indiana University, Bloomington, Ind.

Schultz, Lewis G., Mount Weather, Bluemont, Va.

Wolcott, E. R., Colorado School of Mines, Golden, Colo.

SECTION C, CHEMISTRY

Walker, W. H., Mass. Institute of Technology, Boston, Mass.

SECTION D, MECHANICAL SCIENCE AND ENGINEERING

Humphreys, Alex. C., Stevens Institute of Technology, Hoboken, N. J.

SECTION E, GEOLOGY AND GEOGRAPHY

Brock, Reginald W., Ottawa, Canada.

Cleland, Herdman Fitzgerald, Williamstown, Mass.

Gould, Charles Newton, Norman, Oklahoma.

McCaskey, Hiram Deyer, Manila, P. I.

Miller, Benjamin L., Bryn Mawr, Pa.

Montgomery, Henry, Toronto, Canada.

O'Harra, Cleophas C., Rapid City, S. D.

Peck, Frederick B., Easton, Pa.

Richardson, G. B., U. S. Geological Survey, Washington, D. C.

Veatch, A. C., U. S. Geological Survey, Washington, D. C.

SECTION F, ZOOLOGY

Baird, Robert Logan, Denmark Academy, Denmark, Iowa.

Bancroft, F. W., University of California, Berkeley, Cal.

Barbour, Thomas, Museum of Comparative Zoology, Harvard University, Cambridge, Mass.

Batchelder, Charles F., 7 Kirkland Street, Cambridge, Mass.

Forbes, Stephen Alfred, Urbana, Ill.

Harris, J. Campbell, 119 S. 16th Street, Philadelphia, Pa.

Hay, W. P., Howard University, Washington, D. C.

Lee, Thomas G., University of Minnesota, Minneapolis, Minn.

SECTION G, BOTANY

Harris, J. Arthur, Mo. Botanical Garden, St. Louis, Mo.

Millspaugh, Charles Frederick, Field Museum, Chicago, Ill.

Penhallow, D. P., McGill University, Montreal, Canada.

SECTION I, SOCIAL AND ECONOMIC SCIENCE

Austin, Oscar P., Dept. Commerce and Labor, Washington, D. C.

Blackshear, E. L., Prairie View, Texas.

Chancellor, W. E., Supt. of Schools, Washington, D. C.

Crunden, Frederick M., St. Louis, Mo.

Emory, Frederic, Queenstown, Md.

MacCracken, John H., Syndic of New York University, Washington Square, New York, N. Y.

McKelvay, A. J., Charlotte, N. C.

Norton, J. P., Yale University, New Haven, Conn.

O'Shea, M. V., University of Wisconsin, Madison, Wis.

Payne, Bruce R., Virginia University, Charlottesville, Va.

Wallace, Henry, Des Moines, Iowa.

SECTION K, PHYSIOLOGY AND EXPERIMENTAL MEDICINE

Bancroft, Frank Watts, University of California, Berkeley, Cal.

Brinckerhoff, Walter R., Public Health and Marine-Hospital Service, Honolulu, Hawaii.

Buxton, B. H., Cornell Medical School, New York, N. Y.

Crampton, C. Ward, Public Schools of New York, New York, N. Y.

Hall, Winfield S., Northwestern University, Chicago, Ill.

Hooker, Donald R., Johns Hopkins University, Baltimore, Md.

Langmann, Gustav, 121 West 57th Street, New York, N. Y.

Macleod, J. J. R., Western Reserve Medical College, Cleveland, O.

Mendel, L. B., Yale University, New Haven, Conn.

Meylan, Geo. L., Columbia University, New York, N. Y.

Soper, George A., 29 Broadway, New York, N. Y.

Stahley, Geo. D., Penn. College, Gettysburg, Pa.

Stewart, C. C., Hanover, N. H.

Sullivan, M. X., Brown University, Providence, R. I.

Terry, B. T., Rockefeller Institute, New York, N. Y.

SECTION L, EDUCATION

Brown, Elmer E., U. S. Commissioner of Education, Washington, D. C.

THE SHEFFIELD LECTURE COURSE

Professor Chittenden, director of the Sheffield Scientific School, has just announced the Sheffield Lecture Course for 1907, Friday evenings, commencing on January 18, to be held at College Street Hall:

January 18—'The Bird Life of California,' by Mr. Frank M. Chapman.

January 25—'The Making of Empire: Nome and Dawson,' by Professor Angelo Heilprin.

February 1—'Progress in Food Production, as illustrated by Milk Supply,' by Professor Lafayette B. Mendel.

February 8—'Through the Great Lakes to the Yellowstone,' by Mr. Howard W. DuBois, M. E.

February 15—'The Life and Legends of the Blackfeet Indians,' by Mr. Walter McClintock.

February 22—'Commercial Waterways and their Economics,' by Professor Lewis M. Haupt.

March 1—'Experiences Among the Chinese,' by Mr. Bailey Willis, M.E., C.E.

March 8—'Meteorites,' by Dr. George P. Merrill.

March 15—'Whales and Whaling,' by Mr. Frederic A. Lucas.

March 22—'Earthquakes, Cause and Effect,' by Professor Herbert E. Gregory.

All the lectures are to be illustrated, and the composition of the course seems unusually attractive.

SCIENTIFIC NOTES AND NEWS

The officers of the American Society of Naturalists, elected at the New York meeting, are as follows: *President,* Professor J. Playfair McMurrich, of the University of Michigan; *Vice-President,* Professor D. P. Penhallow, of McGill University; *Treasurer,* Dr. Hermann von Schrenk, Missouri Botanical

Garden; *Secretary,* Professor E. L. Thorndike, Columbia University; *additional members of the Executive Council,* Professor W. E. Castle, Harvard University, and Dr. Charles B. Davenport, Cold Spring Harbor, N. Y. The society will meet next year at Chicago in convocation week.

DR. FRANZ BOAS, of Columbia University, has been elected president of the American Anthropological Association.

PROFESSOR J. H. COMSTOCK, of Cornell University, has been elected president of the American Entomological Society, which was organized in New York City during convocation week.

MARSTON TAYLOR BOGERT, professor of organic chemistry at Columbia University, has been elected president of the American Chemical Society for the year 1907.

DR. WILLIAM BATESON, fellow of St. John's College, Cambridge, well known for his work on variation and heredity, will give the Silliman memorial lectures at Yale University next year. The preceding lecturers on this foundation have been Professor J. J. Thomson, of Cambridge; Professor C. S. Sherrington, of Liverpool; Professor Ernest Rutherford, of McGill, and Professor Walther Nernst, of Berlin.

DR. OTTO LUMMER, professor of experimental physics at Breslau, will begin his course of ten lectures at Columbia University on February 15. Dr. Joseph Larmor, of St. Johns College, Cambridge, will begin a course of six lectures on March 27.

PROFESSOR ERNEST W. BROWN, who goes at the end of the present academic year from Haverford College to Yale University, has been awarded the gold medal for 1907 by the Royal Astronomical Society for his work on the movements of the moon.

THE council of the Geological Society of London has made the following awards: The Wollaston medal to W. J. Sollas, F.R.S., professor of geology at Oxford; the Murchison medal to A. Harker, F.R.S. of Cambridge; the Lyell medal to J. F. Whiteaves, paleontologist to the geological survey of Canada; the Bigsby medal to A. W. Rogers, director of the geological survey of Cape Colony; the Wollaston fund to A. Vaughan, for his work on zoning the Carboniferous Limestone of England; the Murchison fund, to F. Oswald, for his book on the geology of Armenia; the Lyell fund, divided between T. Sheppard, of Hull, Yorkshire, and T. C. Cantrill, of the Geological Survey of England.

AN oil portrait of Dr. J. C. Branner, vice-president of Leland Stanford Jr. University, and lately state geologist of Arkansas, has been presented him by the members of the former survey as an expression of their high regard and of their appreciation of his example and inspiration as a geologist and as a man. The portrait was painted by Mrs. Richardson, of San Francisco.

IT appears from the daily papers that a vote has been taken in Germany on the twelve greatest Germans now living: Dr. Robert Koch, Professor Ernst Haeckel, Professor Konrad Röntgen and Professor Ernst von Behring occupy, respectively, the third, fourth, fifth and eleventh positions in this list.

THE Carnegie Institution of Washington has made a grant of $3,000 a year for a period of four years to Dean W. F. M. Goss, of Purdue University, for the purpose of determining the value of superheated steam in locomotive service; first, in connection with single expansion engines; and second, in connection with compound engines. This is the second grant which the institution has made to Dean Goss. While given to him personally, its effect will be to stimulate and to make more effective the work of the Purdue Locomotive Laboratory. Funds thus received will be employed in supplementing the resources of the laboratory as derived from all other sources. The results of Dr. Goss's previous research under the auspices of the Carnegie Institution, which was for the purpose of determining the value of different steam pressures in locomotive service, are now in press.

KING EDWARD has granted to Professor Sir Rubert William Boyce, Professor Major Ronald Ross, C.B., and Mr. John Lancelot Todd

license and authority to accept the cross of commander of the Order of Leopold II., which decoration has been conferred upon them by the King of the Belgians, in recognition of their valuable services to the cause of medical science.

Mr. E. H. Wilson has been sent by the Arnold Arboretum of Harvard University to make collections in central and western China.

Major C. F. Close and Captain G. R. Frith, Royal Engineers, have come to this country on behalf of the British government to study the methods of the U. S. Coast and Geodetic Survey.

Dr. Hugo Münsterberg, professor of psychology in Harvard University, has returned to Cambridge after a visit to Germany.

Mr. Ernest Chubb, employed in the collection of recent mammals at the British Museum (Natural History), has been appointed assistant curator of the Buluwayo Museum, Rhodesia.

Dr. M. X. Sullivan, instructor in physiology in Brown University, has been appointed an expert in fertility investigations in the Bureau of Soils, U. S. Department of Agriculture.

Professor A. Lawrence Rotch delivered a lecture on the 'Exploration of the atmosphere over land and sea' before the Canadian Institute at Toronto, on January 12. Mr. Stupart, the director of the Canadian Meteorological Office, expects to establish a sub-station at Toronto, and may be able to carry out the wish of the International Committee for Scientific Aeronautics by creating stations for aerial soundings in Newfoundland and Bermuda.

The seventh lecture in the Harvey Society Course will be given by Professor Edmund B. Wilson, of Columbia University, at the New York Academy of Medicine, on Saturday evening, January 26, at 8:30 P.M. Subject: 'Recent Studies of Heredity.' All interested are invited to be present.

On February 1, Dr. A. G. Webster, head of the Department of Physics in Clark University, will give the address on Founder's Day commemorating the anniversary of the birth of Jonas B. Clark.

Dr. George H. Shull, of the Carnegie Station for Experimental Evolution of the Carnegie Institution, spoke before the Columbia chapter of the Sigma Xi Society on January 17 on 'Recent Aspects of Plant Breeding and Variation.'

January 11 was the hundredth anniversary of the birth of Ezra Cornell. The founder's day exercises at Cornell University, however, have been postponed until April 26. It is hoped that Governor Hughes, ex-officio trustee, and formerly of the faculty, will be present and speak.

On the evening of December 31, Dr. John C. Hemmeter presented to the Medical and Chirurgical Faculty of Maryland a life size marble bust of Rudolph Virchow, and made an address on 'Virchow as an Anthropologist.'

The Rev. Dr. James Woodrow, formerly professor of natural sciences in the University of South Carolina, has died at the age of seventy-nine years.

Dr. Wilhelm Königs, professor of chemistry at Munich, has died at the age of fifty-five years.

Advance proofs of the report of the director of the Missouri Botanical Garden show the customary activity and growth of that institution for the past year. Visitors numbered 117,553, an increase of 16 per cent. over the number for 1905, and 36 per cent. more than the average for earlier years—except the World's Fair year. The number of species and varieties of plants cultivated was increased about 7 per cent., to a total of 17,072. Over 300 kinds of chrysanthemums, in 4,000 specimens, were tented in the fall, and viewed by about 17,000 persons. The herbarium was increased about 6 per cent., to a total of 559,267 specimens, valued at $83,890.05. The number of books and pamphlets in the library was increased about 7 per cent., to 54,895; and the total valuation of the library was increased about 5½ per cent., to $89,023.26.

A LETTER to the Harvard College Observatory from the Rev. Joel H. Metcalf, of Taunton, Mass., announces that he photographed the planet Ocllo (475) on 1907, Jan. 11^d, 15^h 30^m.5 G. M. T. in

R.A. 7^h 32^m 17^s.8 (1855).
Dec. + 48° 22′ 58″. (1855).

Note: This object is of interest since its orbit has a greater eccentricity than that of any other known asteroid. A year or two ago it was in danger of being lost. It is now very faint as its computed magnitude at opposition on January 12, 1907, was 14.2. See Harvard Circulars 63, 101 and 103. [E. C. P.]

THE heirs of Dr. J. Brettauer, of Trieste, have presented to the University of Vienna his collection of medical medals. They gave with it a small endowment for the maintenance and enlargement of the collection.

THE secretaries of the American Philosophical Society announce that the general meeting of 1907 will be held on April 17, 18 and 19, beginning at 2 P.M. on Wednesday, April 17. Members desiring to present papers, either for themselves or others, are requested to send to the secretaries, at as early a date as practicable and not later than March 20, 1907, the titles of these papers, so that they may be announced on the program which will be issued immediately thereafter, and which will give in detail the arrangements for the meeting. Papers in any department of science come within the scope of the society, which, as its name indicates, embraces the whole field of useful knowledge. The publication committee, under the rules of the society, will arrange for the immediate publication of the papers presented in the *Proceedings* or the *Transactions*, as may be designated.

THE Pennsylvania Farmers' Congress has passed the following resolution:

WHEREAS, we the members of the Pennsylvania Farmers' Congress in our annual session at The Pennsylvania State College, realizing not only the great work now being done by the School of Agriculture and the Experiment Station, but also realizing that the demands upon these institutions in the near future will be greatly enlarged if Pennsylvania is to assume and maintain the position that naturally belongs to her agriculturally among her sister states.

Therefore, be it resolved that we urge upon our representatives in the Pennsylvania Legislature the most careful consideration of, and the most generous response to, the needs of The Pennsylvania State College for the maintenance of these institutions for the coming two years.

THE Royal Swedish Academy of Sciences will publish early in the present year the first three volumes of Swedenborg's scientific works, edited from the original MSS. in the Library of the Royal Academy in Stockholm, by a committee of the Royal Academy, assisted by Alfred H. Stroh, of the Swedenborg Scientific Association of America. The first three volumes will be: Vol. I. Geology—Introduction by A. G. Nathorst. Vol. II. Chemistry, Physics, Mechanics—Introduction by Svante Arrhenius. Vol. III. Cosmology—Introduction by Svante Arrhenius. These volumes will be followed by others on anatomy with introductions by Gustav Retzius. The volumes are in Latin with the introduction in English. They are the result of the most notable efforts to put in the hands of the scientific world the earlier writings of Swedenborg. A new and revised edition of his *Principia Rerum Naturalium* in English, long out of print, is now going through the press in England, under the auspices of the Swedenborg Society of London.

THE report of the U. S. Geological Survey on the production of gold and silver during 1905, compiled by Mr. Waldemar Lindgren, has now been published. The figures showing the production of gold and silver, in approximate distribution by states and territories, are the result of conference and adjustment between the Geological Survey and the Bureau of the Mint, and are accepted as final by the two bureaus. The total production of gold was 4,265,742 fine ounces valued at $88,180,700; the total production of silver was 56,101,600 fine ounces valued at $34,221,976, making an entire total value of $122,402,676. The production of gold in the United States for 1905 represents an increase of $7,716,000 in value over the production of 1904. The rapid advance in gold produc-

tion which began in 1892, but temporarily halted from 1901 to 1903, was resumed in 1904. This increase in 1904 over the output of 1903 was approximately $7,000,000 and in all probability the increase in 1906 over 1905 will be at least the same amount. The chief sources of the great increase are as follows: Alaska added about $6,000,000 to its output of $9,160,458 in 1904, and Colorado, Nevada and Utah added about $1,000,000 each to their product of the previous year. On the other hand, decreases are noted in Arizona, Idaho, and other states. The states producing over $1,000,000 in gold rank at present in the following order: Colorado, California, Alaska, South Dakota, Nevada, Utah, Montana, Arizona, Oregon and Idaho. The production of silver in 1905 represents a decrease of 1,581,200 ounces in actual output, but in spite of this the increase in the average price 4 cents an ounce (from 57 cents in 1904 to 61 cents in 1905) effected an addition to the value in 1904 of $765,952.

UNIVERSITY AND EDUCATIONAL NEWS

THE *Experiment Station Record* states that a new agricultural college and research institute for Madras is now in course of erection. In 1905 a grant to the presidency by the government of India of $50,000 per annum, which was subsequently increased to $100,000, added to the allotment made by the government of Madras, removed all financial difficulty experienced by the Madras agricultural department. The result of this improved financial condition was the decision of the government to close the agricultural college at Saidapet and establish a new college and research institute, adequately equipped with laboratories and class-rooms and with a suitable farm near Coimbatore. The staff will consist of an expert agriculturist as the principal of the college, a superintendent of the central farm, a government botanist and an agricultural chemist. Ultimately an entomologist and mycologist may be added. The staff will combine teaching with research work. Problems connected with the agriculture of the presidency will be studied in the laboratory and the field, while the students will be given a general education in all branches of agricultural science.

A FIRE, originating in the photographic room of Marischal College, Aberdeen, caused damage amounting to about £500.

THERE will be two fellowships open in the department of zoology and entomology in the Ohio State University for the coming university year. These provide a salary of $300 and cover also tuition and laboratory fees. The holder is expected to carry on graduate work and may be called upon for assistance for laboratory work not to exceed half of the university time. Applications should be addressed to Professor Herbert Osborn, Ohio State University, Columbus, Ohio.

DR. J. H. WRIGHT has been appointed assistant professor of pathology in Harvard Medical School.

MR. BERTRAM G. SMITH, for the past three years an assistant in zoology in the University of Michigan, has been appointed instructor in biology in Lake Forest College, at Lake Forest, Ill.

MR. ALFRED AKERMAN, formerly state forester of Massachusetts and at present state forester of Georgia, has been given charge of the department of forestry which has been inaugurated at the University of Georgia.

THE following appointments have been made at George Washington University: Isaac Wright Blackburn, M.D. (Pennsylvania), professor in Georgetown University, to be professor of morbid anatomy; George Albert Ross, A.M. (Columbian), professor of mathematics in Hardin College, to be instructor in mathematics, and Mr. Abraham Press, to be lecturer in engineering.

MR. W. E. COLLING has been placed in charge of a department of economic zoology at the University of Birmingham.

DR. DEVAUX has been appointed professor of plant physiology, and Dr. Marchis, professor of physics, at the University of Bordeaux.

MR. A. W. ANDREWS, M.A., has been appointed to the new lectureship in geography at University College, Aberystwyth.